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EDITED BY

J. MARK BALDWIN
JOHNS HOPKINS UNIVERSITY

HOWARD C. WARREN
PRINCETON UNIVERSITY

AND

CHARLES H. JUDD
YALE UNIVERSITY
(*Editor of the Psychological Monograph*)

Studies from the Psychological Laborat
of the University of Chicago

CONTROL PROCESSES IN MODIFIED HAND WRITING: AN EXPERIMENTAL STUDY.

BY

JUNE E. DOWNEY
Associate Professor of English and Philosophy, University of Wyoming.

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PREFACE.

I am glad of an opportunity to express in this place my lively sense of obligation toward those whose help has made it possible for me to write the following paper.

First of all, my grateful acknowledgment is due Professor James Rowland Angell, to whose encouragement and instruction I owe not only the completion of the present piece of work but also whatever of pleasure or of profit I have derived from the study of psychology.

Again, I record with pleasure my gratitude toward those members of the Department of Psychology of the University of Chicago who so generously gave of their time and interest in serving as reagents in the experiments to be described. To Miss Florence Richardson special thanks is due, in that she served in double capacity, as subject, and, in the tests on the writer, as experimenter as well.

I desire also to express my thanks to Dr. Grace Raymond Hebard, Librarian of the University of Wyoming, for the help she has given me in the procuring of reference works and for her advice in the preparation of manuscript.

JUNE E. DOWNEY.

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CONTROL PROCESSES IN MODIFIED HAND-WRITING. AN EXPERIMENTAL STUDY.

INTRODUCTION.

1. The investigation recorded below was undertaken in the hope that an extensive and detailed examination of the control processes involved in some particular type of action would yield additional information relative to (1) the nature of the imagery processes in general and verbal imagery in particular; (2) the mode of functioning of these processes, their interplay in a particular concrete situation, specifically in the voluntary act of writing; (3) the relation one to the other of specific sensory processes, auditory and vocal-motor, visual and grapho-motor, vocal and grapho-motor, etc.; (4) the variational factor in the verbal imagery of individuals with its bearing upon the question of mental types in general and attention types in particular. The working out of these problems raised further questions as to the possibility of the initiation of a voluntary act without any sort of a sensorial cue and, also, questions as to the nature of mental distraction, mental lapses, and motor automatism.

As the nature of the experiment necessarily called for introspective reports, it was thought well to choose as subjects trained observers. The subjects serving were either instructors or graduate students in the department of psychology of the University of Chicago. The method used served to a certain extent to check up the reagent's introspections inasmuch as the changes introduced into the situation tended to bring about variations in the modes of response and shifts in adjustment. This method consisted in the writing of a given verse,[1] learned orally, a phrase,[2] and the reagent's name under conditions designed to throw into the foreground different control processes.

In general outline the experiments fell into three series. In

[1] The familiar rhyme, "Thirty days hath September," etc.
[2] University of Chicago.

the first series writing went on under some particular limitation of sight or movement. The subject wrote blindfolded; with left hand; formed some new coördination in writing, such as that involved in mirror-writing and in inverted writing; or wrote with the right hand in a strained position.

In the second series of experiments, writing was accompanied by a distracting process—the term 'distracting' being used here in a purely descriptive way, that carries no theoretical implications. Specifically, the distracting processes consisted in (1) an auditory-vocal-motor distraction (counting aloud); (2) a visual distraction (reading mentally); (3) a visual and auditory-vocal-motor distraction (reading aloud); (4) an auditory-vocal-motor distraction under visual limitation (counting aloud and writing blindfolded); (5) an auditory distraction (the reagent counted mentally the number of times that a particular word occurred in a rhyming list read aloud by the experimenter). In the case of each sort of distraction, except the last, attention was successively (*a*) undirected, so far as the experimental directions were concerned; (*b*) directed by the experimenter to be thrown upon the distracting process; (*c*) directed by the experimenter to be thrown upon the writing.

In the third series of experiments the effect of a time-lapse between the giving of the experimental direction and the writing of the name or phrase was tested. A description was also obtained of the various ways in which it was possible for the subject to image the act of writing the verse. The time required for various methods of ideating the verse was recorded.

Throughout the experiments conditions were kept as constant as possible. Subjects served as far as possible at a regular hour weekly. They used fountain pens, each subject keeping the same pen throughout the series; they wrote on paper of a standard quality and one size; and they maintained throughout the experiments a relatively constant position and angle for the writing pad.

Every experiment was preceded by a test of the normal writing, the speed of which was timed with a stop-watch. Writing under the conditions cited above was also timed. For the name, in addition to the normal autograph obtained each week,

a series of fifty normal autographs was obtained at one experimental session in order to get material from which to reckon the mean variation in speed and in writing amplitude that might be expected to occur during any one hour. In general, however, the time-readings in the following records make no pretense to any absolute value, the number of times any particular test was tried being too restricted to use in any such way.[3] The time-readings are instructive, however, in other connections and in particular cases check the observer's report.

2. It is proposed to begin with a general analysis of the writing-situation with the understanding that no attempt is made to deal with it genetically[4] but solely as worked out in the experiences of adults. It is to be observed, first of all, that this writing-situation is much more complicated as a voluntary act than description is accustomed to recognize. The citing of the four factors in the verbal situation, namely, the visual, articulatory, auditory, and grapho-motor word processes is descriptive of content merely. There is throughout a complication, structurally, of ideational and sensory processes; functionally, of anticipatory and report processes. Yet a preliminary statement is necessary as to the treatment of the various forms of the verbal image in present-day literature.

The verbal consciousness has been subjected to somewhat extended interrogation and in each of its so-called types to

[3] See Kaeding's discussion, including bibliography. F. W. Kaeding: *Ueber Geläufigkeitsuntersuchungen oder Feststellung der Schreibfluchtigkeit der Schriftzeichen* (1898). Kaeding, in his attempt to get at a standard unit of rapidity which could be utilized in rating the stenographic rapidity of individuals, found that the rate of work was modified by the duration and the time of writing test; by the size and such characteristics of writing as pressure on the pen, direction of movement, breaks in continuity; by practice effects; by fatigue and excitement incident to experiment; by variation in conditions during the course of the experiment. See also C. N. McAllister: 'Researches on Movements used in Writing' (*Studies from the Yale Psychological Laboratory*, VIII., pp. 21–63), in which the relative ease of upward and downward movements is discussed in its bearing upon the best movement and slant for writing.

[4] For development of writing in the child, see Baldwin: 'Mental Development,' Chap. V., sec. 2. Goldscheider: 'Physiologie und Pathologie der Handschrift,' *Archiv. f. Psychiatrie*, XXIV., 2, p. 503. Ballet: *Le Langage Intérieur*, Chap. I. For the genesis of writing in the race, see Keraval: *Le Langage Écrit.*

specific investigation.[5] Beginning with the Stricker controversy, there has been prolific discussion as to the possibility of an articulatory image uncontaminated by peripherally-aroused sensations; discussion as to the status of the auditory image as an essential or non-essential part of the articulatory image or as a possible independent existence; discussion as to the possible implication, however marginal, of visual word-imagery as a part of the content of all articulatory consciousness. Silent reading,[6] as well as silent thought, has presented its problems and raised in its turn the question whether meaning can ever attach to a pure visual image apart from motor accompaniments. Writing-consciousness which obviously involves the visual and grapho-motor word-image, also depends upon auditory-motor processes, since thinking, which (copying and dictation apart) initiates writing, usually assumes in its final outcome an articulatory form. Bawden[7] advocates the description of the types of speech consciousness under the two heads, visual-motor and auditory-motor, urging the consideration that the meaning-idea as distinguished from the pure word-idea is always motor. The bearing of the facts obtained in the present series of experiments on the points at issue will become evident as the discussion proceeds.

Meanwhile another line of argument has been opened by Professor Woodworth's conclusion[8] that a voluntary act can be initiated by a 'naked thought,' that is, one void of all sensorial content whatever. Even admitting the concept of imageless thought as thinkable, it may be urged that experimentally at least there could be no demonstration of its sole efficiency as cue to a voluntary act since the very conditions of experimentation would involve sensory material in the form of verbal directions or sense-signals of some sort. Nor even in a self-initiated voluntary act is it clear how sensorial content could be

[5] Stricker: *Sprachvorstellungen;* Ballet: *Le langage intérieur;* Egger: *La parole intérieur;* Dodge: *Die Motorischen Wortvorstellungen,* etc.

[6] Secor: 'Visual Reading,' *American Jour. of Psychol.,* XI., pp. 225–236.

[7] Bawden: 'A Study of Lapses,' *Psychol. Review,* Monograph Supplement, Vol. 3, No. 14.

[8] Woodworth: 'The Cause of a Voluntary Movement,' *Studies in Philosophy and Psychology,* Garman Commemorative Volume, pp. 351–392.

completely eliminated and naked thought isolated introspectively, since by Professor Woodworth's own showing the act is the outcome of the whole situation with all that that involves of sensory (because embodied) consciousness.

Again, Professor Woodworth's strictures[9] as to the adequacy of the image to serve as cue on the ground of its absence of distinctness and accuracy seems due to ignoring the function of the whole situation as defining and giving meaning to the cue, a concession which is by no means equivalent to saying that the situation minus the so-called cue could function in the same way that it does when it is present. The very schematism Professor Woodworth criticises in the conventional treatment of a voluntary act is evident in this inditement of the adequacy of the image, for the image, as he cites it, appears to be only an abstraction, isolated for purposes of structural analysis.

As to the relevancy at all of the kinæsthetic image as antecedent to a voluntary act Professor Woodworth is particularly sceptical,[10] since, according to his showing, such an anticipation is rarely evident to introspection and even when reported is easily impeached on the ground that consciousness has been beguiled into such a belief through a confusion of the kinæsthetic image with sensations 'of initial position or preliminary adjustment of the member' to be moved. So much may be granted. But the further contention, preparatory to denying it any function whatever, that the kinæsthetic image to be conceivable as a cue at all must 'copy' not the movement to be executed but the movement that in a sensational series would precede it and initiate it sensationally has again a schematic sound. A movement may well be initiated by an image of a movement which in a sensational series would precede it—and that too without being a useless duplicate of a sensation—just as in the act of writing the visual cue of a particular letter is often constituted not by an anticipation of the letter itself but

[9] *Op. cit.*, p. 383: "If whatever imagery may be present is less particular than my intention and than the act which results, then the image is not the adequate cue of the act." Also C. T. Burnett: 'An Experimental Test of the Classical Theory of Volition,' *Studies in Phil. and Psychol.*, Garman Commemorative Volume, pp. 392–401.

[10] For a full statement of the argument, see *op. cit.*, p. 272 f.

by the seen or imaged letter that precedes it in the habitual setting (see reports below); but, on the other hand, it is difficult to see why in a world of infinite repetitions and combinations it should be held impossible for an image to form an anticipatory association with the one movement it resembles with more or less fidelity.

It is a consequence of Professor Woodworth's general position that much of the imagery reported by observers as present in voluntary acts is held to be unnecessary and irrelevant. This raises the question, quite apart from the interpretation he puts upon the fact, as to the possible utility of seemingly superfluous imagery. Of which more later.

Apart from such difficulties in his position, Professor Woodworth's analysis of the antecedents of a voluntary act, particularly of the causal value of kinæsthetic sensations of initial position, etc., is very penetrating. Highly suggestive too is his conception[11] of a "set or adjustment, or temporary 'disposition'" of the nervous system 'which can hold over for a while.'[12]

We proceed now to a detailed analysis of the factors which may enter into the voluntary act of writing. The analysis is based upon introspective reports on writing embarrassed by certain limitations in the hope of throwing into the foreground factors of control in a new situation, or even factors of control present in the more habitual process but apt to be overlooked by introspection. It is evident that an analysis to be complete must recognize the whole situation, concrete as well as verbal. Such recognition would demand recall of the visual aspect of the situation such as the surroundings in general and writing-tablet, hand, arm and pen in particular; it would include kinæsthetic sensations arising from the writing posture, hand-and-arm-position and eye-adjustment being important. Such factors, modified by the course of the writing, would persist throughout and might well serve in a directive capacity. Auditory elements in the form of report from the

[11] *Op. cit.*, p. 389 f.

[12] Relative to this holding over of the nervous set, see Experiment 1, Part III., below.

moving pen, or, in the experimental situation, of signals given aurally would also enter.

From the verbal side analysis[13] has concerned itself largely with the visual and grapho-motor processes. The hand-kinæsthetic process is obviously the '*sine qua non*' of writing as an act although not necessarily of the writing-consciousness; for one must distinguish between the writing-movement which might conceivably be a purely physiological event and the consciousness of that movement either in the form of an image of anticipation or of a sensational report of the movement as achieved, either of which might fail to assume kinæsthetic terms. A sensory kinæsthetic report on the movement as it proceeds is of course usually present, although it varies apparently in vividness and accuracy from individual to individual and has a different value for the same individual under different conditions. Its normal presence is testified to by the peculiar feeling that accompanies its lapse from consciousness. This kinæsthetic report has undoubtedly great utility as a conscious control process, probably functioning in the usual course of writing as a corrective but also at times as an initiative process as well.

The grapho-motor process as purely ideational and anticipatory has a more dubious existence and value. The difficulty found in the attempt to identify a pure articulatory image is here duplicated; while again the complication of the latter with auditory imagery is paralleled in this case by the fusion of the graphic with the visual image. As introspection on the part of certain observers has failed to disclose the existence of the hand-motor writing-image, recourse has often been had to pathological cases in which writing has remained possible in spite of verbal blindness and verbal deafness. The following reports offer some introspective evidence as to the reality and nature of the grapho-motor image; for the kinæsthetic factors, both sensational and ideational, are thrown into the foreground when the writing coördination is broken into.

[13] See particularly Ballet, *op. cit.*, pp. 54–58; Dodge, *op. cit.*, pp. 64–70; Keraval, Chap. VI. (interesting for statement of the varying value of the visual element in the writing experience of various races).

Introspection of the nature and function of the kinæsthetic factor is peculiarly difficult yet an understanding of the rôle it plays in consciousness is basal to any treatment of the meaning of automatism. From movement as clearly anticipated and reported in whatever terms, and movement as merely reported 'motorly' there are many gradations to movement which is simply physiological, and the possibility of experimentally inducing such variations in writing offers a particularly promising point of departure for a study of automatism in general.

Another general observation seems in place. Emphasis has often been laid upon the distinction between movement as a mere motor phenomenon and the kinæsthetic content of consciousness, which is as obviously sensory as any other kind of content. Yet for some reason the kinæsthetic image is still treated theoretically as carrying or implying a stronger impulse to movement than does a visual or auditory image, an assumption which only experiment can test. Moreover, throughout discussions there is in general a tendency to confuse functional and structural points of view and to pass from the conception of consciousness as sensori-motor, with all that that implies of motor value for all imagery-processes, to the assumption that every image possesses a motor content. This assumption may be true to fact since, as the image arises only in a consciousness functioning 'motorly,' in genesis and development it must be continually modified by the reaction into it of kinæsthetic material; but it is an assumption which is inadequate in so far as it ignores the fact that this content assumes very different value in the experiences of different individuals and might conceivably approach a vanishing point.[14]

The function of the visual factor in writing considered from the genetic point of view, or where new coördinations are attempted, is obvious enough. Its utility in the fully-formed writing habit is, on the other hand, held to be trifling. Introspection rarely reveals an anticipatory visual verbal image and there is a tendency to minimize the value of the visual perception of the result of the writing movement except as directive

[14] Bawden: "The terms 'Sensory' and 'Motor,'" *Psychol. Rev.*, VII., p. 390.

in the matter of spacing and alignment. Says Bawden:[15] "It is practically impossible to determine with experimental accuracy the degree to which the kinæsthetic is helped out by the visual imagery in writing."

Reference is frequently made to writing blindfolded in order to establish the slight significance of the visual element. What the discussions ignore, however, is the presence in such writing in the case of many individuals, of a distinct visual anticipation or even more often of a visual report. This second process should not be confused with the anticipatory one for when both processes are present they may show distinct differences[16] not only in vividness and spatial position but also in characteristic form or appearance. As to the meaning of the visual '*report*' in blindfolded writing, the author is inclined to think that it indicates the importance of the visual perception of the writing when the eyes are open and shows that the visual report has both a corrective and at times a cue function. At least one subject in the experiments reported below felt confident that he got his cues frequently from this visual report; and in the case of the writer, who is conscious of following writing very closely when eyes are used and who has a vivid visual report when writing blindfolded, the utility of this report is felt to range from giving confidence and keeping the movement from lapsing into an automatic one to actually constituting the cue. The distinctness with which the latter mode of functioning stands out against the ordinary case where the report is felt to be in no sense initiative is rather striking.

Dr. Raymond Dodge[17] in his brief treatment of the visual report during blindfolded writing is inclined to be sceptical of its value and considers it as a mere intensification of the visual anticipation. In certain of the reports its striking incongruity

[15] *Op. cit.*, p. 69. See also statement on p. 68. "Of course the kinæsthetic imagery can not be separated from the visual process in writing any more than from the auditory process in speech."

[16] For instance, a student volunteers the information that in writing blindfolded the visual 'report' appears about an inch to the right of the visual anticipation and is darker in color: S, see below, reports a case where the visual anticipatory imagery appeared above the writing paper, while the visual report appeared on the paper itself.

[17] Dodge, *op. cit.*, p. 65 f.

with the anticipated result makes such a general interpretation impossible. Moreover, Dr. Dodge's own visual report apparently followed the writing movement only in a general way. For certain of the writer's subjects the correspondence was so detailed that they were able to reproduce from the visual report (not the kinæsthetic) minute inequalities in the movement. The tendency for the visual element to function in this way for the kinæsthetic report is characteristic of certain individuals and suggests interesting questions as to the various types of relation between the motor and visual aspects of consciousness. These relations will become more evident as the discussion proceeds.

The writer is also led to believe that visual anticipation, which blindfolded writing throws into relief in the case of a few observers, is indicative of a certain amount of such control in normal writing, for it is found only in the case of those who make explicit use of visual anticipation in more complicated situations. Dependence upon visual anticipation and upon visual report are, it should be noticed, not necessarily found in the same individual, although dependence upon any particular sense element is apt to be a general one. In the case of certain writers, whom experiment has shown to utilize on occasion visual anticipation, the author has been interested in noticing the evident tendency to slurring or telescoping of letters after a word is once under way—as though the intention once visually realized took little account of further execution. The observation has of course only the suggestive value of a very restricted examination of such cases.

The verbal articulatory process, whether or not involving muscular innervation, and accompanied or unaccompanied by audition depending upon the individual, is also a very significant factor in the writing situation. Normally, it is clearly anticipatory of the writing movement, but under certain conditions it seems either synchronous with the writing or, at times, sequent to it, as a report. The difference between the articulatory process as the thought process and as the actual motor cue to the writing is shown in the cases where a double articulatory

process is present;[18] the first outstripping the writing and perhaps using a different verbal unit from the slower process which accompanies the writing at nearer range. Such a possible doubling of the vocal process is clearly facilitated by auditory imagery and perhaps points to the independence of such a process. In any case the significance of the articulatory process, whether indispensable or not, its qualitative characteristics and the scope of its unit are thrown into the foreground by all of the distraction experiments and by alterations in the conditions under which writing takes place.

The various 'controls' thus schematically treated enter into varied and individual combinations with one another. The fusion of vocal-motor and grapho-motor, of auditory and articulatory processes has already been emphasized as more or less evident in the case of nearly every observer. More sporadic and individual were the occasional reports as to the fusion of visual and auditory or of visual and articulatory processes. The whole situation, it should be observed, is one of great complication, a besieging of consciousness by a multitude of sensations and images, so casual and varied as to seem irresponsible, often so evanescent as to raise a question as to their effectiveness. The general problem as to the nature and functioning of consciousness itself intrudes.

3. Following this summary statement of what processes may be looked for in the situation under consideration, a general study of the imagery processes of the different observers, so far as the verbal consciousness is concerned, is presented. Greater value would attach to such studies from the standpoint of individual psychology, if they could be paralleled by a study of the functioning of the imagery processes in a situation organized on a concrete rather than a verbal basis. Unfortunately such a treatment is impossible. Nor if it were within the scope of the present paper would the writer be wise—or daring—enough to complete the study by a statement of each observer's temperamental peculiarities.

[18] Cf. the writer's experience in taking dictation where she hears mentally a sentence or two in the lecturer's voice, while vocalizing mentally in her own voice the word or phrase that she is just on the point of writing.

B. This observer had cultivated auditory imagery and auditory attention to a high degree. His usual writing-cue was strongly auditory-vocal-motor, the auditory aspect of the process being marked. The auditory aspect of the writing-report was also prominent; for instance, the sound of the pen-scratching sometimes served as a guiding process, normally so in making such letters as 'm' and 'n'; and in blindfolded writing this report gave confidence. The kinæsthetic factors of the writing, both sensational and ideational, were also marked. B was confident of the presence in certain cases of an anticipatory kinæsthetic image which seemed purely ideational as evidenced by the fact that occasionally there occurred conflicts of kinæsthetic images or conflicts between an image and the movement actually in process of making, or contemporaneousness of one movement and the image of another, the kinæsthetic cue keeping ahead of the writing (see left-hand writing). The writing process was thoroughly well automatised. B reported at times a lagging of attention behind the movement, a retardation that sometimes produced errors. Even the auditory-vocal-motor accompaniment seemed in certain tests to be largely a report, not an anticipatory process. The visual aspect of the verbal consciousness was much less in evidence. Perceptual visual control was not utilized to any extent while a visual report was very infrequent and, on occurring, inadequate. Visual accompaniments to the writing process occurred, however, in the form of concrete images of the writing situation; position of hand and paper, general alignment, etc. Again, when novel conditions demanded visual readjustments B, showed excellent control of visual imagery. (See experiment 5.)

The distraction experiments, in confirmation of the other tests, showed that while mere visual distraction (reading mentally) caused little embarrassment, the introduction of a lip-motor process caused difficulties, evidenced by oscillations in attention and conflicts in imagery processes. At times B reported that he had succeeded in synchronizing two auditory-vocal-motor processes (one perhaps being ideational and probably purely auditory) and achieved a verbal duet, as it were. The auditory aspect of the two processes differed in intonation.

Attention was enforced usually by concentration on the auditory side of the situation, either the sound of the voice, the sound of the moving pen, or the auditory aspect of the ideational verbal process. Double vocalization sometimes ensued in the effort to hold attention on the writing process; both the word and the letters of the words would be 'auralized,' such images being at times superimposed. Hand-automatism showed itself, evidenced by frequent lapses of the kinæsthetic report and by errors of repetition, incidental to the lagging of attention behind the movement. Only by means of auditory stimulation was it found possible to effect complete distraction. Under such a condition writing became almost thoroughly automatic; it approximated the normal speed; and showed none of the errors resulting from the recall of attention to hand-movement so characteristic of writing under conditions of less perfect distraction. Writing movement under distraction was large and free.

Tested as a guide in an experiment in muscle-reading,[19] B again gave indication of strong tendencies to motor expression. In such experiments, however, in contrast to what has been stated as true in the writing tests, B used visual attention spontaneously, and the voluntary concentration on a vocal-motor process, at the suggestion of the reader, inhibited the motor indications, so far as the 'reader' could judge.

D (the writer). The usual writing-cue of this observer was auditory-vocal-motor, with pronounced auditory aspect, but at times the control became clearly visual, sensational or ideational according to circumstances. D's control over visual images was, however, inadequate[20] as shown by the difficulties encountered in inverted writing and in writing with

[19] In this muscle reading, the experimenter attempted to find articles hidden by the guide. Indications as to the place of hiding were obtained by touching lightly the wrist of the guide. The time required for the finding of each article was recorded. Variations in the test such as blindfolding the guide as well as the reader were employed. The writer is at present carrying out a series of such experiments as a method of getting at the motor value of different imagery processes.

[20] The perplexity which accompanied such actual inadequacy of the sensory cue (*pace* Professor Woodworth) testifies to the motor ineffectiveness of mere good intentions!

the arm in a strained position, situations in which an anticipatory visual control was attempted. Moreover, the visual images manifested a certain amount of 'automatism' or independence. An inverted 'J' which D had vainly sought to visualize when producing such characters surprised her by its emergence during mirror-writing when its services were no longer required. Again, D was once unexpectedly greeted by a visual report that adopted the characteristic appearance of the hand-writing of another reagent (whose chirography D admired!). This erratic and irresponsible character of visual verbal imagery D reports as also a feature of visual imagery in concrete situations. It should also be noticed that occasionally D obtained a clear-cut visual image of a letter inverted or otherwise transformed which she found herself unable to trace. D reported no hand-kinæsthetic imagery. In general, she finds it all but impossible to imagine a graphomotor image;[21] at best she obtains a serial development of visualized letters, the motor value of which apparently arises from a running of the eye over the contour of each letter. The kinæsthetic report from the hand was, on the contrary, often distressingly acute. The hand was frequently cramped. This report was most evident in the tests of normal writing and in distraction tests when attention went to the writing. Mere distraction, however, served to relieve the hand strain and ranged in its effect from a mere easing of the strain to the production of a complete lapse of kinæsthetic report as to the details of letter formation, a lapse which produced a peculiar sense of irresponsibility.

The distraction experiments heightened mental excitement and usually facilitated writing, partly because the distraction of attention from the hand-movement eased the movement. There was at times a rapid flickering of attention from process to process but on the whole the situation under distraction seemed unified and simply one of such complexity as to demand great concentration of attention. Attention was usually en-

[21] A strong tendency to hand innervation is however evident, for severe muscular fatigue of the hand was on one occasion brought on by a prolonged observation of the writing-movements of other reagents.

forced on a particular process by an intensification of the visual '*report*,' sensational or ideational. Counting, which was almost automatic, did not interfere at all with the vocalizing of the writing-cue, for the vocalization involved little or no throat innervation and was described as vocalization principally because of the feeling of agency accompanying the aural process. Visual distraction caused some difficulty, although visual images were able to float evanescently over a visual perceptual situation with coercion sufficient, however, to keep the writing controlled and conscious. Writing, except in the case of name, was not described as automatic. Sometimes there was a blurring of the visual report with the lapse of the kinæsthetic report, but usually the visual consciousness was intense. In general, under distraction, writing rapidity was increased; there are several tests in which the rate is equal to or greater than any which D was able to achieve otherwise, even when voluntarily speeding. In proportion as the experiment involved concentration, or when attention went to the writing, the writing became diminished in amplitude. In certain tests the writing is very minute. In general, there are few pen-lapses.

S. This subject is in general strongly visual in type, both in concrete and verbal experiences. After some practice in introspection he reported an articulatory verbal process as present during writing, although at first he had been inclined to deny its existence and in certain tests its utility is apparently slight. This articulatory process S reports as purely motor, auditory content having in general but little significance for him. Writing is a well-organized process for S, who has done much clerical work and is an excellent and even penman. The interesting feature in his case was the close fusion of visual and grapho-motor processes, isolation of either being at times impossible. Both visual and kinæsthetic anticipation occurred often in control, particularly in novel situations. Report on the movement made was usually visual, and followed in detail the movement actually made. In general, S was not inclined to be automatic in action and oral-verbal expression was particularly irksome.

The distraction experiments were found difficult; time of writing was retarded. S, however, took them very quietly; attention oscillated deliberately from one process to the other; lapses were comparatively few in number. There was little variation in writing-amplitude.

Tested in muscle-reading, S proved a difficult subject. Very slight motor indications were given. Movements suggested by the reader were repeated over and over again. Under such conditions attention was visual.

Y. This observer reported himself as visual in type; and as also greatly dependent upon articulatory processes. Both statements proved accurately descriptive of his writing-consciousness. The loss of perception of the writing result when Y was blindfolded caused more embarrassment in his than in any other case. There was in this and in other tests a recourse to visual anticipation and visual report. Y's vocal process involved actual innervation varyingly of throat, lips and tongue. Y was doubtful as to its possessing any auditory aspect whatever. There was, moreover, accompanying vocalization, a visualization of the position of the organs involved. This visual-vocal process was thrown into the foreground at times. Curiously enough, in his first attempt at mirror-writing, when questioned as to his control, Y denied the presence of vocalization, explaining its absence on the ground that it would be impossible to reverse the vocal cue. In later mirror-writing tests, however, he used such a cue to inhibit the tendency to concentrate on the seen result, which introduced perplexity. Prior to experiment, Y reported that there was little tendency for his actions to become automatic and that his attention could not diffuse itself over more than one process. Hand-writing in particular demands close attention and is much less automatised than is the lip-motor process.

Distraction experiments of whatever nature caused great embarrassment and were very fatiguing.[22] Attention swung deliberately from one process to the other. There were frequent conflicts of throat and hand processes as though it were

[22] This may have been due in part to Y's physical condition at time of experiment.

impossible to innervate both except in a process already unified; vocalization of capitals, particularly, often introduced difficulties. The time required for writing was very greatly increased by distraction and errors were frequent. The need of attention in order to keep writing controlled was shown in Y's failure to write even his name correctly under conditions of distraction. His writing under such conditions shows disorganization and is somewhat reduced in size.

In the muscle-reading test Y proved to be an excellent subject so long as he made use of verbal consciousness or brought his arm into the field of attention. Mere visual attention on the hidden object was, however, ineffective.

R. This subject showed a dependence chiefly upon hand-motor and auditory-lip-motor control in hand-writing. Visual material was sometimes present and could be evoked by effort but seemed of slight value usually. There was a strong tendency to automatism with emphasis on the motor side of the situation. Motor readjustments, however, were effected with great ease in contrast to the case of H, also motor in control (see report below). The distraction experiments caused excitement and often increased speed of writing. Writing often became automatic but remained well coördinated and shows few errors. Attention operated in other cases by a method of exceedingly rapid flickering; there was little report of synchronization of processes. Writing under distraction became very much enlarged.

R's value as a guide in muscle-reading varied. At times there was a pronounced tendency to follow every movement-suggestion given by the reader.

H. Consciousness for this reagent is strongly motor in content, visual and auditory material being usually conspicuous by their absence. H's writing-cue was frequently anticipatory and grapho-motor, H being confident of the existence of the kinæsthetic image. Although the grapho-motor processes, both anticipatory and 'report,' were those most in evidence, the vocal-motor was also present, letter by letter motivation occurring frequently. This articulatory process is reported to be chiefly motor; H questions its possessing any auditory content

whatever. H reports a high degree of automatism generally; she effects motor readjustments with some difficulty.

In general, H found that the distraction experiments involved little effort, although there was a retarding of the writing and frequent errors. The hand-motor series seemed capable of running itself by way of the kinæsthetic report. Occasionally this report lapsed and automatism resulted. The writing process could be set off reflexly; anything in the situation could be used as initiatory cue.

Tested in muscle-reading, H proved to be a most effective guide. The motor indications were strong and unambiguous. When blindfolded, she lost her bearings and was confused by a shifting of the position of the furniture in the room where the test was carried on, although the situation as a whole was a perfectly familiar one. During this experiment, in order to concentrate on the hidden object, H made use of a verbal process (articulatory).

A. This subject is in general type strongly visual, but has also developed to a high degree vocal-motor automatism. Both characteristics were brought out by the tests. Of all the subjects tested, A used the greatest amount of visual anticipation. Unlike the other subjects who are grouped with him he made little use of visual 'report.' A rarely reported a grapho-motor image and had great difficulty in imaging the writing act in terms other than visual. There was little tendency to lapse of consciousness of writing, although A was the most rapid writer of all those who were tested.

The distraction experiments required effort and writing under such conditions was achieved only by oscillation of attention. The tendency to vocal-motor automatism reduced somewhat the value of counting as a distraction.

F and W. It was, unfortunately, impossible to complete the series of tests on these two subjects. The tests of Part I. show that both, to a very high degree, relied, in writing, upon the grapho-motor sensational series. F, however, adjusted herself to the new conditions with astonishing ease; W, with extraordinary difficulty. Both were excellent guides in the muscle-reading test.

PART I.

1. Blindfolded Writing, Right Hand. Elimination of Sensational Visual Control.

Through blindfolded writing one obtains material for estimating the element of control contributed by actual visual perception of the writing movement and its result. Or, to put it negatively, since writing blindfolded serves to eliminate one of the two sensational control processes it tests (1) the loss incurred by such an elimination and (2) the adequacy of the other sensational control (the grapho-motor) to function alone, or, in certain cases, in conjunction with an ideational visual control.

General statements as to the conditions obtaining in blindfolded writing are met with frequently. Professor Woodworth[1] epitomizes the situation as follows:

"These peculiarities of the muscle sense control may be readily tested by writing with the eyes closed. It will be found (1) that at the ordinary speed of writing, a single letter or a short word can be formed as well with the eyes closed as with them open; (2) that, however, when the speed is low enough to permit of fine secondary adjustments, the eyes assist greatly in forming the letters just right; (3) that extreme slowness is a disadvantage rather than an advantage when the eyes are not used; and (4) that if several words are written with the eyes closed, the alignment is lost or some other constant error makes itself evident."

The statement that in general blindfolded writing tests merely the muscle sense control may be questioned. The elimination of the sensational visual element does not cut out the ideational visual control and in the case of the visualizers the experimental conditions actually throw this factor into relief and emphasize its potency. For a more detailed state-

[1] Woodworth: 'The Accuracy of Voluntary Movement,' p. 72 f. Monograph Supplement to *Psycho. Review*, Vol. 3, No. 13.

ment of the writer's view as to the causal value of the visual material occurring during blindfolded writing the reader is referred back to p. 9. Certain reports bring out specifically interesting facts relative to the relation of the visual image and the kinæsthetic sensation. In other cases, in the effort to obtain an adequate control, there is an emphasis of the other sides of the situation, the auditory, for example.

Spacing and alignment from introspective accounts, as well as on experimental evidence, do appear in great measure dependent upon muscular control based largely on sensational experience; individual differences in this respect testify to the differing adequacy of the kinæsthetic sense. (See table, p. 51.) The movement in changing from one line to another is not, it should be noticed, merely a repetition of the movement from left to right. It is an attempt to reproduce the extent of this left-right movement in centripetal terms. Moreover, qualitatively the two movements are very different, since the first includes the kinæsthetic effort involved in the arm-hand-finger movement required in the formation of letters, while the second is a free sweep of the arm from the end of one line to the beginning of the next. In the present test since before writing blindfolded the observer had written the verse several times with eyes open, he might have been expected to possess a more or less accurate motor image of the extent of the movement required in passing from the end of each line to the next. However, the changes in word-spacing introduced such variation as to make the spatial judgment a novel one.[2]

Relative to the kinds of errors students of blindfolded writing have recorded, reference may be made to Crépieux-Jamin's observation[3] of a constant tendency for writing to begin in successive lines farther and farther to the right. (An observation not confirmed in the case of every subject of the present test, although found to be generally true.) Crépieux-Jamin also testifies to the accuracy with which letters and details of punctuation are taken care of. He raises cursorily the ques-

[2] For an experimental demonstration of the nature of such a judgment of movement-extent, see Woodworth, *op. cit.*, p. 7 f.

[3] *L'écriture et Le caractère*, p. 152.

tion as to the amplitude of writing under blindfolded conditions. He is puzzled by the approximation to normal size, because of his expectation that the withdrawal of light—a dynamogenic factor—would result in a decrease in the writing amplitude. His explanation for the failure of such expectation is that the dynamogenic effect of the effort involved in writing thus more than compensates for the loss of light.

Preyer[4] comments on the similarity of writing produced when blindfolded to that written with eyes open, citing as the only differences (1) that words and lines are separated more than the normal extent, due to the writer's anxiety to keep them from running one into the other and (2) the fewer breaks in the continuity of the writing, attributed to the writer's fear of losing his position if he raise his pen.

The results obtained in the present test will be discussed chiefly in connection with the nature of writing control. The tabulated results on pp. 50 and 51 give in figures the rapidity of blindfolded writing, the general area covered, the errors in horizontal alignment, and the errors of line displacement from the vertical. These figures have no absolute value, for the measurements are taken from only one test. (Others were, however, tried and the results are similar in character.) But taken with the other figures given, these measurements indicate certain individual peculiarities. The normal given is that taken on the day of the blindfolded test.

The introspective studies are made from two (or three) trials in writing verse and name blindfolded, compared with reports on a normal taken the same day. In general the tests confirmed the slight dependence, in writing, upon visual perception except as directive in the matter of spacing and alignment. In contrast to the general result, one observer (Y) found that writing blindfolded involved great effort. The embarrassment caused by the absence of perceptual visual control was evidenced by many mistakes in the writing, such as repetition of letters and letter-parts, disorganization in formation of letters and great increase in the time required for writing. The writing-cue of this observer was definitely visual and

[4] *Psychologie des Schreibens*, p. 34 f.

anticipatory. To a less extent the same difficulty was evident for another observer (A) who also reported visual anticipation as his control.

A summary follows of the principal control processes used.

1. *Vocal-motor Control.*—The vocal-motor verbal process, with or without audition, depending upon the observer, was reported by all as present. The verbal unit was usually the word, but with line-unit for one (A) and letter-unit for another (S). Y also made use of letter vocalization after the word was initiated visually; vocalization of capital letters interfered, however, with the writing of them. Letter-vocalization with emphasis on the auditory aspect was also resorted to by B at moments of hesitation. The vocal accompaniment was less prominent when the name was written. It was reported unmistakably by only two observers (R and F); a third (D) made use of a suppressed mental mumble and occasional vocalization.

2. *Auditory control* in the form of a report from the sound of the pen was mentioned by only one observer (B).

3. *Visual* material was at a minimum in the case of three observers. Two reported none except for name (H and F); a third, visual anticipation of only one word, of the spelling of which she was doubtful, and occasional visual report (R). B, who has little verbal imagery in visual terms, except an occasional anticipation and visual report, visualized vividly the concrete aspects of the situation and got visual space values, probably in dependence upon tactile values. The writing of the first word in the verse is often, however, initiated for this observer by a visual cue.

Much visual material was reported by the other four observers. For D the process takes the form of a vivid visual report which is cited as affording moral support. Although this report is not felt to be essential, D is unable to suppress it. There is no visual report as to the appearance of the whole. Normally the report is in her own hand-writing but on at least one occasion it appeared in the hand-writing of another, a hand-writing D was unable to reproduce. Visual control was essential to the other three observers (Y, A, S).

For the first, anticipatory visual verbal imagery served as a cue; this was supplemented at times of hesitation by a visual report. A also made use of visual anticipation but was unable to get space values visually. S used a certain amount of visual anticipation, letter by letter, with a great deal of visual report which was utilized in dotting i's, crossing t's, etc. He reported visualization of the whole situation, both verbal and concrete, and mentioned the interesting fact that the amount of visual verbal report visible at any one moment depended upon the position of the visualized hand and pen, so that moving the hand from the end of one to the beginning of another line increased the amount of writing that could be seen.

It was mentioned in a preceding paragraph that vocal control was less noticeable for name than for verse; visual material, on the other hand, was increased in amount. Every observer mentioned it in the form either of anticipation or report, and either for whole or part. The question arises whether this report of increased visual material may not be due to the fact that such material, being evanescent, escapes the memory and so is not reported at the close of a longer writing-sequence. Or it is possible that the autograph, being as it were one's verbal photograph and social representative, acquires thereby a heightened visual significance.[5]

4. *Grapho-motor Control.*—That motor control is utilized in writing blindfolded is of course evident. Introspective testimony to the presence of this factor was, however, infrequent. Most observers were conscious of utilizing motor and tactile sensations in spacing or, at least, in realizing space-values and in estimating the extent of movement necessary in making the sweep from the end of one line to the beginning of the next; one observer spoke of the eye-strain involved in lining.

An attempt was made to have the subject reproduce with eyes open certain words of which he had had a visual report when writing blindfolded, with the object of testing the ade-

[5] For an interesting treatment of graphology as applied sociology, see article by Tarde, 'La Graphologie,' *Revue Philosophique*, XLIV., p. 337.

quacy of this report as a translation of the grapho-motor series. In the case of the name, a pronounced size illusion appeared for D[6] and R. Capitals, particularly, were reproduced in characters much larger than the originals. A slight illusion of the same nature appeared also in the reproduction of the first verse-word (B).

A curious relation between the visual and motor situation was reported by Y, in that movement towards the beginning of the line in returning to the left was checked by the visualized edge of the paper. Y was distressed by the shortening of the movement this image effected. The same observer remarked, relative to an error in crossing a capital 'T' as though it were an 'F,' that the 'T' was reported visually as correct, although the motor report testified (and accurately) to the presence of the cross-piece; in the second test in connection with the same letter, Y, aware of an intention to cross the 'T,' was not aware that he had done so; but in witness to the embarrassment of the situation was the persistence of the visualized 'T' throughout the writing of the rest of the line. B also cited a case in which the visual report announced a letter as complete, although the motor report felt inadequate. As a matter of fact the letter was telescoped.

The loss of visual perceptual guidance is evidenced by wide spacing of lines and words, of which several observers were aware, but used as a resort to avoid running words into one another; by defective alignment, both horizontal and vertical; and in one case by degeneration in the formation of letters. For a numerical statement of errors see the table on p. 51. In summary, it may be said briefly that the error in horizontal alignment is for four observers a fall from the initial level; for three others the error varies from one of ascent to one of descent; for the remaining observer the error is uniformly a slant up—although this subject reported that the lines ran downhill. Progressive displacement of lines farther and farther to the right (under-estimate of extent) was evident in

[6] The amount of this illusion, D has since discovered, varies with variation in direction of attention. It is greatest when attention is focused on the forearm movement; least when attention is focused on the finger movement or the visual report.

the writing of four subjects; for two others the displacement was to the left; and for the remaining two it varied in direction. This error of progressive displacement is evident even in normal writing and in the case of one subject (D) persists in spite of an endeavor to correct it.

The amplitude of the writing was considerably increased for Y, and slightly for R, H and S.

Blindfolded writing was used also in connection with left-hand writing, mirror-writing and 'speeded' writing; it was also used in connection with certain distractions. The results obtained from these different experiments should be compared. Compare also Plate I., Nos. 1 and 3.

2. Left-Hand Writing. Embarrassment of Grapho-Motor Control.

As the object of the test was to get a statement of variation in the sensory control in adults when the customary grapho-motor coördination is embarrassed, no discussion of spontaneous left-hand writing is called for. One trial each in writing the given verse, the phrase, and name was made (1) with eyes open, (2) with eyes closed. (Cf. also left-hand mirror-writing.)

As might be anticipated the verbal unit actually functioning as grapho-motor cue is, in the case of left-hand writing, more fragmentary than in the case of right-hand writing. In the case of at least four observers the actual cue came one letter at a time.

1. *Vocalization Process.*—The utility of this process ranged from the one instance in which apparently it was the only motivation to hand-movement (R), to its marginal presence. The observer reporting the articulatory process as the only cue, used it letter by letter. The fact that writing blindfolded she was able to do the verse in 67.8s., less than half the time required by every other reagent, taken in connection with the illegibility of the letters, which were scarcely formed at all, tends to substantiate her introspection. In another case, that of H, writing was preceded by a grapho-motor cue and also accompanied by letter by letter vocalization. For other reagents word by word

vocalization accompanied writing with occasional recourse to spelling.

2. *Visual Material.* (*a*) *Perceptual.*—The value of the visual perception of result in controlling movement was very evident in writing with the left hand. It was no longer a case of the eyes being used merely as an aid in alignment and spacing; they were directly instrumental in controlling the formation of letters. The least utilization of visual perception was shown by R. Introspectively she reported but slight dependence upon vision; writing, whether eyes were open or closed, was motivated by the spelling out of the words. The increase in legibility and coördination with the use of the eyes, although not as pronounced as in the case of the other subjects, testifies, however, to a certain amount of visual control. As before, the rapidity of this subject is in excess of that of the others. F made so little use of visual report that only after writing some time was she aware that she was not doing it mirror-fashion.

(*b*) *Ideational.*—This control was particularly noticeable in the blindfolded writing, but occurred also in anticipation in writing with eyes open. Three observers motivated left-hand writing mainly by the visual cue. There was a double visual series, both anticipatory and sequent to writing, the two visual series not always being in agreement. A reported, moreover, an actual fatiguing of the eyes by the writing, in contrast to the hand and arm fatigue mentioned by other observers. He noticed a strain of the eyes toward the right, particularly in writing the last two lines of the verse, lines which were unusually long. This strain of eyes at the periphery is noticeable in ordinary writing by A, who wears glasses. The interesting point, however, is the fact that he had difficulty in visualizing the words at the outer edge of the sheet. The writing at this place is badly disorganized. Such disorganization is, it is true, found in the writing of every observer, for movement at this point becomes difficult on account of the cramping of the forearm as it is drawn in toward the body. Further interest attaches, however, to the visual embarrassment. Was it a translation of the motor difficulty? Y motivated his writing by

visual anticipation, letter by letter, following a vocalization of the word. This visualization is reported at times as a visualizing of the required right-hand movement and includes a picture of the hand as well as of writing. This process is the obverse of that noticed by Y in blindfolded writing, right hand, where visualization of the word was followed by letter by letter vocalization. The change in control is evidently due to the break-up of the motor habit. S reported a visual anticipation, letter by letter, each seen as though made by the right hand (S utilized as copy the hand-writing of a penman whose chirography he had often imitated). This visualization of letters was accompanied by a grapho-motor image (with perhaps hand innervation) of the letter as made by the right hand, a motor image then translated into left-hand terms. S cites also a visual report which he judged, in case of blindfolded writing, functioned as an important part of the cue. The other observers report visual ideational control in varying degree; none for R and F; slight visual report, H; letter by letter visual report, B; strong visual report, D.

3. *Grapho-motor Control.*—In four reports there is introspective evidence of dependence upon grapho-motor anticipation as cue to the movement. S, as given above, used grapho-motor anticipation in connection with visual cue, translating the right-hand movement into a left-hand movement letter by letter. H likewise cited a motor feeling anticipatory to the making of each letter; this plus the letter-articulation constituted her cue. F was preoccupied with the motor side of the situation and was inclined to believe that there were anticipatory grapho-motor images. The clearest-cut case of conscious grapho-motor control is found in the blindfolded writing of B. Motor images were used as cue throughout. These anticipations ran ahead of the writing by two or three letters. Writing with eyes open, however, B reported less grapho-motor anticipation, for the visual report, used here to check up results, introduced a certain amount of conflict between report and movement, and the auditory cue became accordingly more pronounced.

The visual-motor conflict just mentioned is reported also by

another observer, Y, for whom the visual anticipation of right-hand writing issued in a movement lacking the expected ease; this conflict threw into prominence the kinæsthetic report from the hand.

The object in beginning with a blindfolded test of left-hand writing was partly the hope of chancing upon a case of spontaneous mirror-writing; therefore no directions were given as to the edge of the paper at which to begin writing. Only two subjects showed any hesitation. H debated at which margin to begin, but finally decided in favor of the left. F tried at first beginning at the left, then at the right, then at the left again, and finally at the writer's suggestion (given indirectly) wrote from right to left through the first line, then began again at the left. It was not until she had finished the verse (left-right) that she became aware by an inference that the line as written first was reversed in appearance. The right-left movement she considered a much easier movement than the left-right. During the writing of the verse there were frequent reversions to such a movement.

D showed some tendency to mirror-writing; a tendency which had to be inhibited in both blindfolded and eyes-open test; in both cases the visual checking of the grapho-motor impulse was quite evident. The illusion of size reported for this observer (blindfolded writing, right hand) was pronounced in writing with left hand, eyes closed; the visual report gave a doubling of actual size. The return to conventional form of letters was also noticeable, an evidence of increase in amount of visual control. See Plate III, 6 (*a*), 6 (*b*).

For specimens of left-hand writing, see Plate I, Nos. 2, 4, 4(*a*) and 4 (*b*).

3. A. Mirror-Writing. Right and Left Hand. Embarrassment of Visual and Grapho-Motor Control.

Mirror-writing was introduced into the present series of experiments with no intention of testing the various hypotheses cited in explanation of spontaneous mirror-writing. It was employed solely as a device for investigating the individual

variations relative to the sort of control that would be utilized in mirror-writing induced voluntarily.

In such a connection it is obvious that only experimental reports on voluntary mirror-writing are pertinent to the present discussion. The questions whether or not spontaneous mirror-writing be a pathological occurrence; whether or not it be the normal writing for the left hand of all persons, or the normal left-hand writing for the left-handed only, need not be entered upon. Further, hypotheses that explain mirror-writing through a theory of 'cross education' or through the greater ease of centrifugal or abductive movements, or find an explanation for it in the child's attempt to copy, identically, with the left hand the seen movements of the teacher's right hand, will be considered only incidentally in connection with the results of the present experiment.[7]

It should, however, be noticed that, in general, writers on the subject agree that mirror-writing is the more apt to result, the more the visual factor falls into the background, whether this occurs because of mental blindness or because of the impairment of attention, habitual or momentary. Mirror-writing is particularly likely to occur if such inattention carry with it an ignoring of the requirement that writing should be visually significant. It is quite in accord with such a conclusion that the slight tendency to spontaneous mirror-writing that was noticed in the present series of experiments occurred in just those subjects in whom visual control, perceptual and ideational, was, in general, least in evidence.

A detailed account of experimental work on voluntary mirror-writing may be found in the article by Abt, cited in the foregoing footnote. Reference to this article, so far as it deals with voluntary mirror-writing, will be made freely and the conclusions there published will be compared with those reached as a result of the present investigation.[8]

In the present experimentation on mirror-writing four tests

[7] See, particularly, Weber: 'Spiegelschrift,' *Ztsch. f. klin. Med.*, XXVII., p. 260 (1895); Wegener: 'Die Spiegelschrift,' *Ztsch. f. Päd. Psychol.*, I., p. 254 (1899); Laprade: 'Contributions à l'étude de l'écriture en miroir' (These Med., 1902); Abt: 'L'écriture en miroir,' *Année Psychol.*, VIII., p. 221 (1901).

[8] See also Laprade, *loc. cit.*, p. 45 f.

each were tried on writing verse and name: viz., mirror-writing, left and right hand, the reagent being blindfolded; and the same test with left and right hand when the reagent had his eyes open. Preceding these experiments there was a blindfold test with the left hand, with no instructions given for direction of writing, in the hope of chancing upon one or more cases of spontaneous mirror-writing. In this preliminary test, only two subjects (H and F) showed the least hesitancy as to direction of movement. Abt accounts for the relative infrequency of spontaneous mirror-writing, left hand, by the fact that visual representation accompanies the writing and consequently under all conditions writing remains under visual control. As has already been stated, conditions favored mirror-writing in the case of the two reagents just mentioned, both of whom made little use of visual material.

In the present series the first test on mirror-writing was made with the eyes closed in order to ascertain the adequacy of the visual cue, if it occurred, and the distinctness of the visual 'report,' if any, in comparison with the perception of the result of the movement when the eyes were open. No directions were given to the subject other than instructions to write from right to left. Several subjects were wholly unacquainted with the nature of mirror-writing and the tests upon them afforded excellent material for estimating the accuracy with which motor sensations were translated into visual terms; two of the observers (H and D) were somewhat practised in mirror-writing. In any case, habituation to the movement was very rapid.

As in the case of left-hand writing (Test 2) the articulatory process was pronounced only for certain observers. B reported distinct vocalization, evident particularly in blindfolded writing with the left hand (first test), in which test there was subdued vocalization of each word with superimposed spelling out of the longer words. R also reported vocalization throughout and spelling out of the longer words. F used, in the first test, vocalization letter by letter; and, in the later tests, word by word vocalization, insisting that the vocal was her only cue. The other subjects were preoccupied with vis-

ual or motor material; articulation if present was marginal. Abt's subjects, on the other hand, in general reported letter by letter enunciation preceding the tracing of isolated letters, each of which was the object of a special act of attention, the word having lost its unity.

Relative to the sort of control utilized by his subjects Abt grouped them in three classes:[9] (1) those who before writing represented the form of letters in mirror-writing; (2) those who were dependent upon a visual representation of the symmetrical movement; (3) those for whom, apparently, the auditory-motor image immediately evoked the movement. The validity of the control cited by the third group Abt questions, being inclined to attribute such a report to faulty analysis; for, he asks, if the auditory motor image calls forth left-hand mirror-writing, how can it call forth by the same mechanism right-hand mirror-writing?

The types of actual control, Abt thus reduces to two, both of which are visual. For the voluntary mirror-writer, according to his explanation, is in the position of a child learning to write. The first condition of such an achievement is to have a visual representation of the appropriate movement which, he adds, is the only precise notion we can have of a movement —a statement open to question. A visual image of the reversed letter may, however, take the place of the visual representation of the required movement and such a substitution gives us the type of control of group (1), to which only three of Abt's thirty subjects conformed. This method of initiating mirror-writing was found to be a slow and laborious one. The writer was apt to become confused and there was frequent reversion to normal writing, either for the whole or a part of a letter. Dependence upon visual representation of the symmetrical movement was the usual method of control. The mistakes of those using such control, unlike those of group (1), were rarely errors of reversion to normal writing and never of partial reversion of a letter; the mistakes that occurred were due to the letter by letter method and included omission or doubling of letters. In general, Abt holds that the more diffi-

[9] *Loc. cit.*, p. 228 f.

culty a subject has in abstracting from his visual imagery, the more difficulty he has with mirror-writing.[10]

Of the eight observers tested in the present series only one (A) used an anticipatory image of reversed letters, converting the visual into a motor image with a little visualizing of the hand in movement and of the moving pen-point. A second observer (S) used visual anticipation after the first test but apparently received initiation into the required movement through motor processes. For the first writer the production of mirror-script was a labored process and there were frequent failures to reverse either the whole or a part of a letter. On the whole the reports from this observer would throw him into Abt's first group.

The writer questions, however, the possibility of classing all the other observers under the second description given by Abt, where visual representation of the required movement served as cue. For why may there not be a motor as well as a visual representation of a movement? In the cases under consideration a visual representation of the movement was rarely spoken of, but grapho-motor control was frequently insisted upon, sometimes as a matter of anticipatory imagery. Certain facts to be mentioned later, together with results from other tests, in which writing is modified, led the writer to accept the possibility of a purely motor representation of the movement.

To cite again particular cases. Three observers (F, H and R) reported scarcely any visual material in the nature of an anticipation, or, when blindfolded, of a report. F who wrote rapidly and with ease insisted (cf. Abt's third group) that the vocalization of the letters served as sufficient cue to the writing and that no visual control whatever entered into the process. Throughout tests requiring inversion or reversal of writing she initiated the process with a conscious intention (perhaps put verbally) of moving the hand in a certain direction. After the writing was once initiated the habit-

[10] *Loc. cit.*, p. 224. Cf. Baldwin: *Mental Development*, p. 99 f., and Goldscheider: 'Physiologie u. Pathologie der Handschrift,' *Archiv f. Psychiatrie*, XXIV., 2, p. 503.

ual motor automatism was adequate, and there was very little anticipation of any sort. R had a slight visual report from blindfolded writing but depended largely upon motor automatism, writing in the first test rapidly and with the same slurring of details noticed above in her left-hand writing from left to right. H also reported motor control and her slight dependence in general upon visual consciousness would confirm her report.

It is interesting to note that the mirror-writing of the last two observers mentioned shows the error of incomplete reversal of letters, cited by Abt as found only in the writing of those initiating the process by a visual anticipation of the reversed letters. On the face of it, indeed, one does not see why breaks of such a kind should not result from a rapid automatic as well as a slow visual reversal.[11]

B reported anticipatory motor cues with no consciousness of reversal except of the general direction of movement and in the case of a few perplexing letters. Occasionally B made the letter in the air normally and then reversed the movement. Writing was accompanied by a visual report. Once a capital letter, an initial of his name, was anticipated, visually in one style and 'motorly' in another, B finally yielding to the motor anticipation. Such a conflict shows the relative independence of visual and motor control. Y also used motor control and his writing was accompanied by a visual report, which he found distressing since it gave a peculiar feeling of mental reversal. The movement itself was easy. As to the adequacy, in the last case, of the visual report from the movement, it may be mentioned that although Y had a distinct enough report on each letter serially, so that he was able to reproduce his name as it had appeared to him when writing blindfolded, the appearance of the whole was surprising and novel.

[11] D had practised mirror-writing so frequently that her report can scarcely be compared with those of the other subjects. If her memory be correct, however, she learned mirror-writing by writing on a blackboard with right and left hand simultaneously, fastening attention on the visual report of the right hand. Such a method, according to Abt, is valuable only for those who belong to his first group.

S reported for the first test an anticipatory right-hand motor cue which was reversed and so converted into a left-hand mirror-writing cue. This motor cue, judging from other tests upon S, may well have been a visual representation of the required movement although he did not so describe it. The interesting feature of this particular case is that after the first test, in which control was spoken of as motor and in which the writing of each letter was followed by a visual report of its appearance, S began to utilize as anticipatory cues the store of images furnished by the movements reported in such terms. In all tests after the first he had an anticipatory visual image of each letter reversed and also a visual report of the letters as written, the latter extending often several letters back.

In general, writing with the eyes open gave results somewhat better coördinated than the blindfolded writing. Alignment and spacing were improved. Only one subject (Y) complained that the presence perceptually of the result of movement was troublesome, and he succeeded finally in ignoring the tracery by concentrating on the vocalization of the verse.

Abt's discussion includes some interesting observations relative to the comparative characteristics of right-hand and left-hand mirror-writing. Right-hand mirror-writing, he observes, is steady and calligraphic, but lacks the appearance of a running hand. There are frequent breaks between letters, the strokes are short and the letters end abruptly. It is retouched frequently and is vertical or slants from right to left in contrast to the slant of normal right-hand writing. Left-hand writing is hesitant with slender wavering strokes, has blunt angles and is elongated and pointed. Its connections, however, are made with ease, it is a more rapid hand than right mirror-writing and has more of the characteristics of the running hand, although it does not resemble the normal right-hand writing.

The samples of mirror-writing obtained in the present test follow the above description only in the case of certain individuals. The description covers excellently the characteristic differences of the writing of the two hands in the case of A and R. Running writing appeared for the right hand in several of the other observers. Interestingly enough the ob-

server most practiced (D) wrote a running hand in both cases; the two hands resemble one another closely and are like the ordinary left-right tracings of the left hand.

Another distinction between left-hand and right-hand mirror-writing, given by Abt, relates to errors in alignment. Left-hand mirror-writing is found to ascend, as is normal right-hand writing, both movements being similar in so far as they are, relative to the body, centrifugal movements. On the other hand, right-hand mirror-writing, together with left-hand unreversed writing, descends, both movements being centripetal. These statements were by no means uniformly true for the subjects under discussion, as is evident from an examination of Table III. given below, in which the error in alignment is given for right and left hand in both the centrifugal and the centripetal movements, eyes open and closed. The individual characteristics are interesting, as is also the exceeding accuracy of the alignment, vertical as well as horizontal, shown in certain cases of mirror-writing.

Left-hand mirror-writing has often been cited as an evidence of cross-education or transference of practice-effects to symmetrical parts of the body. It is so considered by Davis,[12] as well as by others before him. It is not clear in this connection why insistence should be laid upon the ability of the left hand to write reversely while its ability to write from left to right is ignored. The former, it is true, is held to result from the writer's surrender to motor sensations which in such cases are dissolved from the 'optical percept' (Davis is citing Goldscheider[12]) and is the more readily achieved the less the writer is under the influence of the 'optical percept.' The seeming implication of such a conclusion is that left-hand unreversed writing remains under the direction of the visual percept or image and that the transferred motor education, if there be such, must be symmetrical.

Abt, in rejecting the hypothesis of cross education, interpreted as meaning the education of a cortical center through the development of a symmetrical center, rehearses as adverse facts

[12] Davis: 'Researches in Cross-Education,' *Studies from the Yale Psychol. Lab.*, 1896–98, Vol. VI., p. 6.

the following:[13] (1) simultaneous right-hand writing facilitates left-hand mirror-writing only when the subject belongs to his (Abt's) first type, control visual; (2) when the two hands write together the movements are successive rather than simultaneous; (3) left-hand mirror-writing is symmetrical to the usual right-hand writing in no exact sense; they are unlike in appearance; (4) the right hand also acquires mirror-writing with great readiness. All of these facts contribute to Abt's conclusion that a visual representation of the required movement comes into clear consciousness in mirror-writing and is directive. A pure motor image is impossible.

So far as the present experiments bear upon the point in controversy, they tend to show that in the case of any particular observer there is a tendency for similar motivation for left-hand writing, both centripetal and centrifugal, although it is true that grapho-motor control has with certain subjects acquired in mirror-writing a value not reported in unreversed writing. It is not merely a question of visual control for the identical and of motor control for the symmetrical movement. In opposition to Abt the possibility of a purely grapho-motor control, either sensational or ideational, is maintained. And it appears from Table II., which gives the time-readings for the various tests, that, so far as rapidity of adjustment to the conditions is concerned, a grapho-motor control is more effective than a visual control. But in opposition to the theory of cross education it is urged that such a grapho-motor control is evident in left-hand unreversed as well as in left-hand mirror-writing and also in the *right-hand mirror-writing*.

Summary.—In accord with the literature of the subject a tendency to spontaneous mirror-writing was most evident in those subjects who made least use of visual control. When, however, the reagent started out with the conscious intention of writing mirror script with either right or left hand, visual anticipation of the letter-form, or visual representation of the movement required, were possible methods of initiating the movement. The present test shows in addition the possibility of grapho-motor anticipation and suggests also the possibility

[13] *Loc. cit.*, p. 250 f.

of a grapho-motor automatism which can function in the reverse. (Cf. Test 4.)

With the exception of one subject (H) grapho-motor control, anticipatory or automatic, was, so far as rapidity is concerned, more effective than visual anticipation. With reagents so initiating movement there is, however, a greater dependence upon a vocal-motor cue than is found in the case of the other subjects. Practice effects mask the possible variation in rapidity, due to the shift from left to right hand or from closed to open eyes. In general, the right hand acquired mirror-writing as readily as did the left. (Compare Plate I., Nos. 1, 5, 6, 7, 8, 7*a* and 8*a*.)

3. B. Reading Mirror-Writing.

Reading mirror-writing involves the utilization of many complex mental habits. It is a much more complicated affair for most people than is mirror-writing. The records on spontaneous and pathological cases of mirror-writing show that the writers are unable to translate their productions unless this habit be deliberately acquired. That is to say, facility in writing mirror-fashion does not carry with it ability to get meaning visually from such script. This fact is, indeed, urged against attempts to explain pathological and spontaneous mirror-writing as due to visual difficulties, presentative or representative. The test was introduced in the present series with the hope of establishing some relation between the imagery processes used in the reading and writing of reversed script, and particularly to determine whether motor tracery would be utilized in the attempt to interpret such visual signs.

Method.—The script used in the reading test was prepared by the experimenter. The same sentence was submitted to nine reagents and the time and method required for its interpretation recorded. A second sentence, similar in character and containing the same number of letters, was prepared for the experimenter, D. The sentences chosen were of such a nature that the context was somewhat difficult to get.

Result.—The greatest individual difference found was the extent to which the context was utilized. Certain reagents

(B, A, F), given a few letters, were able to construct the meaning of the whole. Others (W, D, and especially H) received little help from the meaning of the sentence.

Again, great differences were evident relative to the range of variation within which certain letters, or even words, were recognized through their similarity to normal script. For all observers certain letters were visually significant even in their reversed form; but the number of letters so recognizable varied greatly from individual to individual. For R many words were recognizable as wholes at the first glance.

Visual reversal of letters was attempted by most reagents, but unsuccessfully. B, however, utilized a backhanded visual reversal, as it were; for the reversed letters would suggest a normal letter which would then be reversed and compared with the given letter. This reagent, with the exception of R, made the best record at the test. In general, visual material was utilized very unequally by different reagents. A letter or word once recognized assumed a fixed value only for certain subjects; others repeated the original process of interpretation.

There was less recourse than the writer had expected to tracing the letters in the effort to interpret them. Such utilization of motor material would perhaps have been greater if each subject had been engaged in deciphering his own handwriting, although mirror-writing does not bear the marked individual stamp of ordinary chirography. H, who found the test extraordinarily difficult and who failed to get any meaning from the sentence as a whole, is strongly graphomotor and attempted to use motor tracery in translation of the writing but gave up the attempt because the writing was so unlike her own that it offered no point of contact. D also attempted to recognize letters by tracing them, but was unable to do so although strongly motor in writing mirror script. Occasional use of motor material was made by B, W and H; but the value of the method was, on the whole, insignificant.[14]

[14] Some of Abt's subjects resorted to motor tracery in deciphering mirror-writing (*loc. cit.*, p. 238 f.). Abt, however, holds that this reading was not through the mediation of the movement sensations, but rather through the visual representation of the movement of the hand. The method was more successful when the tracing was actually done on paper, a mode of tracing not attempted by the subjects in the test above.

Habitual methods of apperceiving certain symbols often introduced difficulties in interpretation. A similarity suggesting a false interpretation would cause more disturbance than a symbol that at first bore no similarity mark and was worked out with effort. The word 'at,' which reversed resembles a 'to,' caused great trouble. For S 'h' reversed was persistently read as 'd'—which it is in German script. H and Y were unable to apperceive a reversed 'f' as anything but 'J,' although the letter occurred in the word 'of' and the context was unmistakable; by means of the context Y finally succeeded in supplying the letter, but H completely failed to do so.

The time taken by the different subjects in reading the mirror script follows. The time-readings are not wholly comparable, for Y and H wrote down letters as soon as recognized and thus had a slight advantage, visually, over the other subjects who read the words aloud.

Subjects.	Time required to read sentence in mirror-script.
R.	40s.
B.	1 minute, 14s.
F.	1 minute, 35s.
A.	3 minutes, 10s.
W.	7 minutes, 30s.
S.	9 minutes.
Y.	9 minutes.
D.	16 minutes, 51s.
H.	Over 25 minutes.

Conclusion—In general, the test had little bearing on the topic under consideration other than showing that the greater mental readiness and facility in adaptation shown by certain subjects in the more distinctly motor tests is parelleled by the result from this test, which is sensory in nature. If the time-readings given above be compared with those in Tables I. and II., it will be seen that there has been little shifting in the relative position of the nine subjects, if they are ranked according to the quickness with which they adjusted themselves to the test-conditions.

4. Inverted Writing, Right-Hand. Embarrassment of Visual and Grapho-Motor Control.

A variant on mirror-writing was introduced by a test in which the subjects were asked to invert writing, that is, to write upside down. The subject was allowed to follow his own preference in beginning at the left or the right margin of the paper. Those most dependent in this test upon visual anticipation in every case but one choose to begin at the right edge. A simple visual inversion was more readily 'imageable' than a reversed inversion; and in fact by a mental *tour de force* certain observers were able at times to see the result of this upside down movement as upright. In general, there was a gain of visual over grapho-motor control, if this test be compared with that of right-hand mirror-writing. The test, on the whole, was more difficult than mirror-writing, although individual differences were more pronounced than in the preceding test. A glance at the time-readings given in Table I. (p. 50) shows at once the relative ease with which any particular observer achieved success in the undertaking, although the fact that the writers did not all begin at the same edge of the paper and write in the same direction makes intercomparison somewhat less valuable. The variation in rapidity is great in any case, ranging from less than *five* to over *twenty-two* minutes. The difficulties experienced by particular observers in this test are comparable with those observed in the succeeding one. (See p. 43 f.)

F, who did the writing in the record-time (4m. 27s.), inverted writing with ease, using either hand and beginning indifferently at either right or left margin, although the left-right writing took 44s. longer than that running from right to left and was, F reported, a trifle more difficult, since if she became 'mixed' in the movement, it was more difficult to reëstablish it because of its greater likeness to normal movement. F reported also word by word vocalization but no visual material of any sort. She started writing simply with a definite intention of reversing up and down directions. Errors were reported in kinæsthetic terms and were usually corrected. F wrote the words as wholes, and when she became confused and

baffled in the process, she would cross out the word as a whole and begin it anew. The writing was a running hand. (Plate II., 1.)

R, whose time is only a trifle longer than F's (5m. 9s.), wrote from right to left and produced a much finer copy than did F with no corrections. In appearance the writing is similar to R's right-hand mirror-writing and is a round, vertical, well-formed hand. The first two words were initiated by visual anticipation of letters afterwards inverted, but after R had once caught the required movement, visualization was resorted to only occasionally.

A similar appearance is presented by the inverted writing of B, who, unlike the subject just named, wrote from left to right. For this subject a visual cue was necessary in order to get a word going; after the word was started, movement-reversal was sufficient. The letters visualized were seen originally above the paper, and were then turned over on the paper and traced. There was some tendency in the last line for B to see the writing reported as upright.

Y, writing from right to left, combined visual and motor control. Y was unable to make the required movement until he had formed a picture of the letters. Many letters were visualized upright and then turned over, but Y found it very difficult to visualize details. He did not, moreover, trace the visualized letters. In the case of one letter he was able to see it inverted correctly, without being able to copy it. At several places there was a tendency to see the writing upright. The writing is well-formed and running.

S also wrote from left to right an even running back-hand script, using visual anticipation for every letter; a letter was first seen upright, then inverted and finally reversed and the image actually projected on the paper. After a letter had been made once, no further visualization was required; the grapho-motor cue was sufficient. When asked to write his name from right to left, S found the process much easier than that used in writing the verse, simple inversion being sufficient. The report from this test should be compared with that of the observer when doing mirror-writing. In the latter

case practice effected a shift from grapho-motor to visual control; in the present test there was a tendency for the visual to yield to the motor. (Plate II., 2.)

The five observers just mentioned produced good copy with few errors; the four remaining subjects were less successful and the writing of one (W) is undecipherable.

A used visual anticipation; some letters were visualized originally as turned over; some as upright and afterwards inverted. Closing the eyes increased vividness of imagery. The writing shows frequent failures to invert.

D found the test one of very great difficulty. She was unable to form a motor idea of the required movement and her visual imagery was inadequate. She attempted visualizing letters upright and then inverting them but was unable to effect the inversion. She also attempted to invert the seen movement of the hand used in tracing letters in the air. This method was ineffectual. Finally D succeeded partially by imaging words on paper right side up and then tracing them deliberately from the other side of the line.[15] This involved a splitting of attention, so to speak, the tendency throughout being to see the writing as normal. This effort of attention was accompanied by a twist of the head to the side and over the writing. This writing runs from right to left and required frequently the inhibition of a mirror-writing tendency. There are, moreover, frequent errors of reversion to mirror-writing. Inverted writing from left to right was, however, for D 'unimageable.' (See below for account of development of habit of inverted writing.)

H wrote from left to right and used grapho-motor control, a reversal of the normal motor cue. The process was a laborious one and there were words not inverted. H spoke of the difficulty of keeping her place in writing and in two cases there are word-substitutions.

[15] Relative to the dependence of such work upon the manner of apperceiving the situation may be mentioned the case of a girl, whom the writer tested, who labored painfully over the production of inverted writing (right-left), until she discovered that the writer—who sat opposite to her—could see the characters upright. She exclaimed, "I'll see them from your side of the table," and began to write rapidly.

W found the experiment almost impossible. Although he declined to begin at the right edge of the paper he frequently made the letters from right to left. The collection is literally one of letters and is a motley array of upright, inverted, and reversed inverted letters. W used no anticipatory visual cues but thought that at times he made use of the perceptual visual process in correction. He often made movements in the air and then tried to reverse them. The difficulty of the process he judged to lie in the inhibition of old writing-habits. (Plate II., 3. Compare Plate I., Nos. 1 and 9, and Plate II., 1 and 2.)

5. Writing in Strained Positions of Arm and with Variation in Direction of Movement.

In this test the reagent wrote his name in several abnormal positions of hand and arm, the direction of writing, left-right, up-down, etc., also being varied. The positions assumed were those required in writing on a tablet placed upon the forehead,[16] on top of the head, parallel to, or at right angles with, the back, or over either right or left shoulder. Not every combination of writing-direction and position was tried with each observer.

An introspective account of writing-cue and report under such conditions of writing was asked for and, as a check on such an account, the subject was asked to reproduce, in normal position, the results obtained in writing under the test conditions. The results are highly characteristic of the different observers and test the adequacy with which the writing control of any particular reagent is able to function under new conditions. The cues here utilized are, largely, visual and grapho-motor. The immediate reaction of the several observers to experimental directions, which were given simply in terms of place and direction of writing, indicated at once control tendencies, for whereas the visualizers were ready at once with questions as to the point of view from which the writing was to be read, those more motor in control accepted the directions as sufficient. The reproductions vary from observer to observer. There is, for

[16] See Wegener: 'Ueber recht-u. rückläufige Stirnschrift,' *Ztsch. f. Psychol. u. Physiol. der Sinnesorgane*, XVI., p. 190.

example, reproduction of the visual anticipation, in which, since the reagent mentally assumed a position that would enable him so to view it, the writing is normal in appearance throughout the series; there is reproduction of visual report in which movement is translated into visual terms in such a way that the appearance of the writing varies, with position of the arm, from normal to mirror or inverted writing; there is reproduction on the basis of a purely kinæsthetic report, a reproduction that, visually, resembles the preceding type but is differently conditioned.

The individual reports in this series are given as meriting particular attention. The results should be compared with those obtained from the same observer in the case of inverted writing. In any one case there will be found the same ease or embarrassment in the manipulation of motor or visual control.

B utilized visual and motor material in both anticipation and report. There was also an auditory-vocal-motor accompaniment which was felt to be a non-essential element. The visual control of the situation was complete. B was able, for instance, to see the result of writing as it would appear if the tablet were transferred from the given position to the writing-table, the whole transfer being in concrete visual terms; or he was able to see the writing as it appeared in the given position viewed from any particular point in the situation. In executing movements some interesting adjustments occurred. For example, when writing from left to right on top of the head B found the anticipated arm-movement impossible and there was a visual and kinæsthetic shrinking of the whole situation; the initial capital shrank from an inch and a half to half an inch in height. Again, in writing on the forehead, down-up, there was a kinæsthetic conflict, apparently ideational, between tensions in the arm at this level and when resting on table.

D showed a general dependence on visual control, but the control was inadequate. D found herself at times unable to place the visual image in the desired position preparatory to tracing it. Visual and kinæsthetic report were also inadequate, so that in reproduction of the result, D frequently had recourse to memory of the visual cue. In the simpler experiments where

the tablet was placed over left shoulder (writing right-left) or tablet placed at back (writing right-left, pencil toward body) there was no visual cue, except perhaps for the first letter; accompanying the movement, however, there was a fairly adequate visual report, reproduced without difficulty. With the transfer of tablet to vertical plane on forehead (writing up-down) a visual cue, letter by letter, appeared. The difficulty was extreme. D was unable to begin movement until 'J' was projected on paper in desired position. Before D succeeded in getting an upright 'J' it was seen in inverted mirror-writing, perhaps in fusion with kinæsthetic image of the same. At the final letter, a 'y,' there was a conflict between visual and kinæsthetic control with final surrender to the kinæsthetic, which proved to be wrong. The reproduction was on the basis of neither visual nor kinæsthetic report, the first not having been present and the second having escaped memory; it was simply a repetition of visual anticipation. When the tablet was placed flat on the face and D instructed to write down-up, she found herself greatly embarrassed. It was found impossible to get the visual cue in position for running movement and finally the hand was allowed to engineer itself, which it did with some success. The visual report was very distinct, but D was unable to reproduce it. In the next test, in which tablet was placed parallel with back (writing right-left, pencil pointing from body), D again yielded to motor control and was unable to retain visual report long enough to reproduce it. This motor control is not anticipatory and D has no sense of agency in connection with it; it is accepted as a last resort. In the next test, in which tablet was again parallel to back, D found herself unable to visualize situation, but decided to attempt mirror-writing, since directions were to write left-right. The result was a most peculiar conflict between kinæsthetic sensations and visual report. There seemed to be an actual split in the process, that is, the visual images and grapho-motor sensations were reporting a double, not a single act.

S The reports from this observer are of particular value as affording illustration of most intimate fusion of motor and visual imagery. S reported anticipatory visual cues which,

however, under the conditions holding in certain of the tests, appeared, not in normal, but in reverse or mirror-writing. The writer is inclined to think that such reversal of the visual cue (?) was due to anticipatory motor imagery, or, possibly, innervation, so that the visual translation of the motor anticipation actually ran ahead of the writing. S, however, was not inclined to accept such an explanation, for the visual image did not seem to him dependent on motor anticipation. One particular test furnished a variation of control which the writer interprets as confirming her position. S was writing from right to left with the tablet placed on the top of his head; the visual cue, letter by letter, was seen in reverse, S looking at it as if from below. Suddenly, the last syllable of his name, 'land,' appeared as a whole in normal writing on the tablet which apparently had shifted to a vertical position on the forehead and was looked at as if from the front. The change seemed due to report of strain from kinæsthetic sensations and with the visual shift the movement was eased. This means, according to the writer's interpretation, that up to the point of transfer, the visual material represented a pseudo-control only; but with the embarrassment of the motor situation it became actually directive. S's reproductions were usually on the basis of the kinæsthetic report and consequently, as the conditions varied, changed from normal to mirror-writing.

Y This observer, like the preceding one, had anticipatory visual images of letters in reverse. These images, however, in the more strained positions of the arm were reduced in value and were reported as dependent upon anticipatory grapho-motor cues. Occasionally Y found himself unable to visualize letters before his arm was placed in position and at times he allowed his hand to do the work without anticipatory control of any sort. Besides visual cues, he also had visual reports, the two at times not agreeing. In one instance Y stated that cue and report were seen from different positions; the cue was seen from the side, the report from in front, an angular difference of 90°.

A The control for this observer was undoubtedly visual anticipation. A questioned the experimenter in detail as to point of view from which writing was to be seen. In every

case he assumed mentally a position from which writing could be seen as normal, although in the first trial (writing on forehead right-left), there was at the start a tendency to visualize mirror-writing perhaps in fusion with anticipated movement, a tendency checked by A's transferring himself to a position in front of writing. Every reproduction is in normal left-right writing with very little attempt to reproduce irregularities of movement. There was not a scrap of articulatory accompaniment to writing. The usual method involved the tracing of the already visualized signature projected in the required position. The projection was not always easy for A. Up-down writing on forehead was motivated by visual imagery that seemed fluid and unconvincing; and in the down-up writing in same situation A visualized first from the left and found the movement impossible, then from the right with motor success. On writing on top of head, front-back, a marked movement-illusion appeared, an error in direction equivalent to 90°. A reported that the writing was seen as though he were looking down upon it from the left and that he found it very difficult to visualize from this position. (See below, where this illusion is discussed in connection with other reports.) In this particular case the question arises as to the relation between the visual embarrassment and the motor error. Since, after the first letter, A resorted to a right-left signature in the usual plane, it is possible that the visual difficulty with subsequent visual shift caused the error.

R A surrender to motor automatism was here evident, anticipation of any sort being infrequent although R occasionally spoke of motor cues. There was little visual imagery except in the simpler tests of writing on forehead, right-left and left-right; in the first case slight visual anticipation, and in the second, a little visual report. Hand and arm once placed in position the movement followed automatically. There was little detailed kinæsthetic report; at most, R spoke of certain movements as being easier than others and once or twice she reported herself unable to reproduce exact movement. Throughout the test she was unaware of any reversal of movement, so that all reproductions are in normal writing. It may

be stated that R's results, from the standpoint of legibility and coördination, are among the best obtained.

H In the case of this reagent, again there was complete absence of visual material. Control was grapho-motor; at times anticipatory. Reproductions were on the basis of kinæsthetic result. The variations from the preceding report are given, as being interesting. Writing on tablet placed on forehead both left-right and right-left was reproduced in mirror-writing. H was puzzled by the fact that the movement-report for both signatures should be similar although the hand had moved in different directions. She was unable to put the situation in its visual terms. The third time of writing name on forehead right-left, she observed that she would write so that writing could be read from in front. This writing was done with great painstaking and consciousness of movement and at least three letters were written left-right instead of right-left. The reproduction of this, from the kinæsthetic report, resulted again, to H's great surprise, in mirror-writing. The reproduction was detailed even to the reversal of the three letters mentioned above. H was unable to see that the change in position of the tablet from forehead to table explained the result visually. Again, reproduction of the signature on top of head back-front gave normal writing, but not until H saw her own reproduction of motor report was she aware of what the visual appearance was.

F This observer gave results in certain respects unlike those of any other reagent. She followed directions unhesitatingly; the process was automatic with anticipatory motor cue for general direction of movement and occasional motor cue and report at difficult places. In attempting reproduction of result, she had neither visual cue nor visual report to guide her, nor did she attempt a reproduction of motor experience. By an exceedingly rapid process of inference, carried on mentally in motor terms, with perhaps a faint visualizing of hands in process of movement, she transferred the tablet from the given position to the writing-table, in such a way as to discern the lay the signature must have on the paper, if she were to reproduce the appearance of the original in the given position. The

result is that, visually, the reproductions copy, in appearance and general direction, the originals when transferred to the writing-table, and are unlike the reproductions of other observers.

W The control for this reagent was grapho-motor but he reported a tremendous break in the coördination, a break which was evident in the outcome. To reproduce the movement at all, W was often obliged to repeat the movements in the air, reproducing the autograph usually in sections. He resorted to such a method even in the reproduction of left-right writing on the forehead and was surprised at seeing the normal signature which resulted.

In writing front-back with tablet placed on top of head, an error of 90° in direction of movement was reported for A, under the same conditions an error of 45° in direction occurred in the writing of H and S. The reverse occurred in the case of Y; although there was no error in the actual direction of writing, he reported, on the basis of the motor sensations, that such an error in direction of movement had occurred.

In conclusion, to make the comparison of individual results the easier, Table I. is appended giving a statement of the appearance of the name when reproduced, a statement followed by a phrase showing the method used in reproduction. The simpler tests of writing left-right and right-left with tablet placed on the forehead are chosen as sufficiently characteristic. It should be recalled that the writing from left to right on the forehead, although requiring no change in movement, appears, when the tablet is transferred to the table, in mirror script. On the other hand, right-left writing, although requiring a reversion of movement, when written and transferred to table, appears normal. The reproduction of signature will differ depending upon the method employed. Visual anticipation, visual report and grapho-motor report may all be utilized in reproducing the writing.

For purposes of further comparison, in the fifth column of the table is given the time required by each reagent to write the test-verse in inverted characters, followed by a statement of the principal control used.

TABLE I.

Reagent.	Reproduction of Writing on Forehead.			Inverted Writing.	
	Direction of Writing.		Method Used.	Time.	Writing Control.
	Left-right.	Right-left.			
B.		Normal	Visual transfer to table	6m. 25s.	Visual and grapho-motor anticipation.
D.	Normal	Mirror	Visual report	11m. 58s.[17]	Tracery in inverse of upright characters.
S.	Normal	Mirror	Grapho-motor report	7m. 22s.	Visual anticipation, requiring both inversion and reversion.
Y.	Normal	Mirror	Visual report	9m. 2s.	Visual anticipation.
A.	Normal	Normal	Visual anticipation	9m.	Visual anticipation.
R.	Normal	Normal	Grapho-motor automatism	5m. 9s.	Automatic inversion.
H.	Mirror	Mirror	Grapho-motor report	8m. 45s.[17]	Grapho-motor inversion.
F.	Mirror	Normal	Motor transfer to table	4m. 27s.	Automatic inversion.
W.	Normal		Grapho-motor report	22m. 13s.[17]	Grapho-motor inversion.

TABLE II.

RAPIDITY:[18] VERSE.

	Right Hand.						Left Hand.			
	Normal.		Mirror Writing.		Speeded.		Normal.		Mirror Writing.	
Eyes.	Open.	Closed.	Open.	Closed.	Open.	Closed.	Open.	Closed.	Open.	Closed.
B.	75	82	176(3)[19]				180	200	184(2)	202(1)
D.	75	83.2	194(4)	128(2)	59.8	49.6	199	155	209(3)	199(1)
S.	72	81	260(3)	229(2)			230	203	250(4)	297(1)
Y.	78.5	90.5	304(4)	406(2)			240	261	255(3)	540(1)
A.	57.2	69	274(3)	385(2)	38.8	43.4	180	187	360(1)	
R.	74	75	176(3)	210(2)	55.4	53.8	143	67.8		137(1)
H.	85.4	80.4	324(3)	386(2)	52.2	49.2	240	203		334(1)
F.	61	77	176(2)	171(3)	56.4	60	185.4	279	164(4)	192(1)

6. Tables II. and III. give in tabulated form the rapidity with which the test-verse was written in the blindfolded, left-

[17] These reagents report a lack of what is known as 'sense of direction.' An inquiry into the functioning of imagery processes at moments when directions of movements are confused would probably yield interesting results. The above test, with that described in 5, is capable of much greater elaboration than was made of it in the test cited. Cf. Binet's 'Illusions of Reverse Orientation,' *Psychol. Rev.*, I.

[18] The readings are given in seconds.

[19] The numbers in parentheses give the place of the particular test in the mirror-writing series.

hand, and mirror-writing tests, with measurements for the area covered in writing the verse under the above conditions together with errors in alignment.

Table III.

Measurement of Area Covered in Writing Verse, with Errors in Alignment.

See note for key.

Eyes.	Right Hand.						Left Hand.			
	Normal.		Mirror Writing.		Speeded.		Normal.		Mirror Writing.	
	Open.	Closed.	Open.	Closed.	Open.	Closed.	Open.	Closed.	Open.	Closed.
B.	144x65[20] 4 A[21] 1-V[22]	140x135 28 D 3.4 L	190x80 8 A 2.2 L				170x76 3.8 A 3 R	168x156 18 D(2) 8 A(4) 6 V	190x95 4 A(4) 2 D(2) 2 L	230x180 22 D(4) 10 A(1) 15 V
D.	162x63[20] 5 A[21] 3 R[22]	180x110 5 D 9 R	156x66 2 A(3) 2 D(1) 1 R	170x166 40 D 9 V	204x76 2 A 4 R	204x115 12 A 25 V	172x76 6 D 1.2 R	218x160 23 A(4) 20 D(1) 7 V	206x65 5 A(2) 2 D(2) 5 L	230x160+ 62 D 4 V
S.	149x56[20] 4 D[21] .6 R[22]	189x113 30 D 12 R	120x50 3 A 1 L	130x124 7 A(3) 5 D(1) 17 V			118x50 5 A 1 R	132x95 9 A(5) 2 D(1) 8 V		216x127 17 D 17 V
Y.	116x76[20] 1.3 D(4)[21] 3 A(1)[22] 1-V	200x210 14 D 10 R	196x75 2 D(3) 6 A(2) 2 V	210x132 8 A(5) 2 D(1) 7 V			174x85 4 A(3) 2 D(2) 3 R	222x170 15 D 9 V	200x93 10 A 5 L	240x160 78 D 17 V
A.	140x78[20] 2 D[21] 1 L[22]	218x124 12 D(3) 9 A(2) 10 V	169x106 3 D 1 L	172x121 29 A 4 R	154x96 1.4 A 4 V	156x92 5 A(2) 1 D(2) 6 V	42 A(5)	168x134 42 A(5) 5 D(3) 13 V	185x73 4 A(5) 3 D(1) 2.4 L	
R.	177x84[20] 2 D[21] 5 V[22]	196x110 25 A 7 R	184x100 10 D 2 V	176x164 15 D 4 V	204x94 .7 V 2 V	218x149 26 D 11 R	180x112 17 D 6 V	198x170 176 D 17 V		222x116 9 A(3) 2 D(2) 6 R
H.	154x53[20] 3 A[21] .6 R[22]	199x150 7 A(3) 31 D(3) 2.3 L	195x93 5 D(3) 2 L	Measurement impossible. 32 A(3) 20 D(3) 7 V	181x56 4 A 2 V	230x185 16 D(4) 10 A(1) 6 R	202x78 2 A(4) 2 D(2) 2 R	225x151 13 A(4) 9 D(2) 8 V		250x155 Irregularity too great to measure.
F.	182x90[20] 8 A[21] .8 V[22]	226x116 9 D(3) 4 A(2) 13 V	194x95 4 A(3) 6 D(1) 4 L	198x102 12 A 12 V	220x96 7 A 2 R	230x170 18 A 20 D(1) 7 V	187x87 4 D 3 A(2) 2 V	260x140 15 D 9 V	157x88 17 A 2.3 L	245x144 33 A 5 V

7. *Conclusion.*—The results obtained from the different tests are, in the case of any one individual, strictly comparable. The main individual variation was the extent to which the subject had recourse to grapho-motor control, either conscious

[20] Whole area covered in writing verse; measurement in mm.

[21] Error in horizontal alignment; average of six lines, measurement in degrees; A = ascending, D = descending.

[22] Error in displacement of lines; measurement in mm.; L = left, R = right, V = variable. Average from five lines.

or automatic. Such a control was the normal one for F, R, H and B. A, D, Y and S, on the other hand, were more dependent upon visual material. Variation occurred within the groups as given. Grapho-motor control was more conscious for B and H than for R and F. In the second group, the visual control was for A, anticipatory; for D, a control in terms of visual 'report.' Y and S frequently reported a visual control that assumed both anticipatory and report forms. Y and S also reported at times conscious anticipatory grapho-motor images, a report which rarely occurred in the case of A and D.

The conditions under which writing went on influenced somewhat the nature of the control utilized. Writing blindfolded caused little alteration in the normal control for members of the first group, whose writing is organized chiefly on a grapho-motor basis. Under like conditions, members of the second group reported much visual material and in at least one instance, the loss of visual perception caused degeneration in the appearance of the writing.

In left-hand writing the characteristics of the two groups remained unaltered except that for the first group grapho-motor anticipation was more frequently reported than before. Such anticipation in ideational terms was particularly noticeable in the case of B.

Mirror-writing increased the tendency to rely on a grapho-motor control, a surrender partly to motor automatism but partly also a control by motor anticipation. Members of the second group (S and Y) made more use than before of grapho-motor control. A alone depended wholly upon visual anticipation.

In inverted writing, in contrast to mirror-writing, there was a gain of visual over motor control. This was evident particularly in the case of B and R, members of the first group. Cases occur as before of surrender to motor automatism and such a method of control was the most effective, if rapidity in writing be used as a standard. Extraordinary difficulty in inverted writing was found in the case of D, H and W. This difficulty seemed independent of the nature of the control

utilized and was paralleled by the difficulty the same reagents experienced when attempting to translate mirror-writing.

Writing under a strained position of the arm and in various directions gave the same individual results as the other tests. Great differences were found in the ease with which different reagents were able to orient their movements in these experiments, in their ability mentally to shift their position in space without confusion. B and F gave greatest evidence of ease in spatial adjustment; the first had recourse to visual, the second to kinæsthetic imagery. D, H and W were spatially confused by the tests.

The results of the tests raise the question of a motor automatism which can function although the direction of movement is altered. Such automatic reversal and inversion of direction of writing was evident particularly in the case of F. Other reagents, who found an initial cue necessary, were able to proceed with the word or line with ease when once the movement was initiated. Emphasis is laid upon this point because it raises the question involved in 'cross education' in a different way, for it is equally evident in the case of the right- and of the left-hand movements. Again, it bears perhaps some relation to the general question of orientation. H, W and D, who found movement reversal particularly difficult to achieve, also report frequent difficulties in general bodily orientation.[28]

[28] D, after the experiment in inverted writing described above, practised the feat for some time. Skill in visualizing inverted writing came rather speedily, although at date of writing it remains still impossible for her to visualize first upright and then invert. Skill in writing inverted characters (from right to left) came slowly and developed only to a slight extent. With such development in skill, however, just the condition reported by F was noticed. If the first letter of a word was once successfully initiated by a visual cue, the movement-reversal of the rest of the word ensued automatically.

A pretty confirmation of this conclusion occurred September 19. Up to this time inverted writing from left to right had been for D a 'sealed book' owing to the absolute impossibility of her visualizing such writing. The day in question, however, she conceived the plan of writing the first word of the test-verse right side up and then inverting and turning over the paper upon which the word was written and tracing the word in transparency in the hope of continuing spontaneously with the next word of the verse. In spite of visual bankruptcy and utter failure to ideate such a movement 'motorly,' D succeeded in continuing the movement thus initiated. Such a method involved automatic inversion and reversion, that is, a turning over of the control of movement to an old habit which functions in a novel direction.

PART II.

Analysis of Writing Control by Means of Distraction.

1. Statement of Method.

Distraction[1] as an experimental method has been used in various connections. It has been shown to be of dubious value if used in the hope of getting quantitative statements as to the attributes of attentional states. For (1) distraction is usually partial and discontinuous, and interstitial flashes of attention are often sufficient to direct the process from which attention is supposedly abstracted; and (2) any given process has a very different distracting value for different individuals, depending in part upon the general mental value of the distraction process itself, and also in part upon the subject's method of handling the double process in terms of imagery or of attention-functioning. But an individual's method of response to any particular distraction is in itself significant, and while the search for a method of complete distraction bids fair to continue for some time, yet the search has already yielded valuable information relative to imagery and attention types. It was with such a purpose that it was used in the tests, the results of which are given below.

Specifically, the distraction experiments served several ends. Any particular distraction either (1) introduced conflict of imagery and so threw a particular control process into the foreground; or (2) caused a 'shunting' of the control and so tested the various possible 'controls' of the subject tested; or

[1] For a critical examination of distraction methods see Alice J. Hamlin, *Amer. Jour. of Psychol.*, Vol. VIII., pp. 1–66.

Distraction has been used in investigation of 'Muscular Memory,' T. L. Smith, *Amer. Jour. of Psychol.*, Vol. VII., pp. 453–490; to test the possibility of 'Visual Reading,' Secor, *Amer. Jour. of Psychol.*, XI., pp. 225–236; to discover the nature of the modification of one psychological process by the coincident presence of a second, Binet, 'Concurrence des États Psychologiques,' *Revue Phil.*, XXIX., pp. 138–155; to discover characteristics of 'Normal Motor Automatism,' L. M. Solomons and G. Stein, *Psychol. Rev.*, Vol. 3, pp. 492–512; to produce lapses to be utilized as material for an experimental study, Bawden, 'A Study of Lapses,' *Psychol. Rev.*, Monograph Supplement, Vol. 3, No. 14.

(3) issued in actual distraction of attention from writing for a longer or shorter interval, so that writing approached automatism.

In order to test the possibilities of control in the case of any particular reagent it should be noticed that complete distraction of attention is by no means necessary. Conflict of processes with consequent oscillation of attention is quite as instructive; for such conflict throws into relief and permits description of the disturbed process. Again, the possible objection that the particular 'control' utilized in any test is an outcome of the experimental conditions and in no way tests the control process in ordinary writing may be met by pointing out the uniformity in the results from any chosen reagent in spite of the variation in experimental conditions. Or, put negatively, the different distractions make evident the processes that are not possible 'controls' for the subject tested. In the case of a subject for whom there are several possibilities of control, the 'shunting' of the control as conditions change is highly instructive. Those reagents who achieved a synchronization of different processes carrying different meanings furnish interesting material for a study of the mode of functioning of attention and the nature of the meaning consciousness, as well as giving valuable information relative to the specific imagery processes involved.

The following distractions were used: I, counting aloud (auditory-articulatory distraction); II, reading mentally (visual distraction); III, reading aloud (visual, auditory and articulatory distraction); IV, counting aloud, blindfolded (auditory-articulatory distraction with visual perceptual control eliminated); V, counting mentally the number of times a particular word occurred in a rhyming list that the experimenter read aloud (auditory distraction). In these tests as in those of Part I, the subject wrote the verse there cited, his own name, and the phrase 'University of Chicago.'

The experimenter in all tests timed the writing during distraction; the normal time of writing, with which that of the distraction test is compared, having been obtained before the distraction test began. In the case of every method of dis-

traction, the experimenter could estimate the rapidity and accuracy with which the distracting operation was carried out. In the case of test II (mental reading as a distraction), the reagent was required to point out the last word read and to identify, very shortly after the test (at most a few seconds) the list of words read. For this purpose the list was slipped at random among a number of lists similar in appearance and to a certain extent identical in wording. When reading constituted the distracting process, lists of unrelated words were used so that the process would not be facilitated by context.

Immediately at the close of each test the subject volunteered an introspective report, which the experimenter recorded. The experimenter also questioned the subject, asking practically the same questions of each subject. Particularly was the reagent questioned as to whether he was aware of any error in writing, and, if so, how he was conscious of it. His report in this case could be checked by the actual result. In the case of all distractions but those of V, the test was repeated three times, unless omitted through accident or unavoidable interruption. In the first test in each series, attention was undirected so far as instructions given by the experimenter were concerned; in the second test of the series, attention was thrown on the distracting process; in the third test, attention was thrown upon writing. Practice and fatigue effects fell upon the same place in every series, but to a certain extent the preliminary experiment, attention undirected, allowed the subject to 'warm up.'

Before a discussion of the results is attempted, the experimental records for the tests upon the verse are given in full with a table that shows the comparative results from the different subjects! For the tests on the name, results are tabulated separately for each subject with a verbal summary of result. In addition to the facts recorded for the verse, in the case of the name, measurements in mm. were taken of the writing amplitude.

2. Individual Reports of Distraction Experiments. Writing of Verse.

SUBJECT B.

(Average time of writing verse, perfectly memorized, for eight trials, 65.5s; mean variation, 4.15s.)

I. Counting Aloud and Writing. Eyes Open.

(Normal time of writing for day, February 26, 69.8s. Normal rapidity of counting, 130 in 60s. One trial each for writing and counting.)

A. *Attention Undirected.*

One trial.

(*a*) Experiment difficult but amusing. Oscillation of attention evident. Conflict noticeable between counting-articulation and vocalization of words written. There was a feeling of strain in arm as though movements were being made with the left hand; also a keen consciousness of tactile and kinæsthetic sensations.

(*b*) Writing-cue was auditory-vocal-motor. Visual and hand-kinæsthetic cue occurred rarely.

(*c*) Time required for writing, 130.8s. Number counted, 120. Lapses: three words are blurred in consequence of correction of some indistinguishable error.

B. *Attention on Counting.*

One trial.

(*a*) Oscillation of attention with conflict of vocal-motor processes. Attention was enforced on counting by throwing it on the sound of the voice and the throat tensions. B was conscious of the hand when writing coördination went adrift. Visual perceptual process was used in correction of errors, although B thought that, on the whole, the test would be easier if the eyes were not focused on the writing.

(*b*) Writing-cue was auditory-vocal-motor; there was little automatism.

(*c*) Time required for writing, 138.4s. Number counted, 130. Lapses: several corrections were made in writing but it is impossible to determine the character of the errors.

C. *Attention on Writing.*

One trial.

(*a*) B counted in a very low voice, in one place inaudibly. Attention was not strongly focused on writing. In the writing of the first three lines there was some fluctuation of attention; with the fourth line the auditory aspect of the writing-cue emerged clearly and from this point on there was little conflict. The counting at this point became automatic.

(*b*) The writing-cue was auditory-vocal-motor, with strong auditory aspect from third line on.

(*c*) Time required for writing, 117.5s. Number counted, 107. Lapses: three, corrected.

Summary.—The writing-cue (cf. cue for name under same conditions, Table VI.) was auditory-motor vocalization, a process which conflicted with the counting. The perception of the result visually was not felt to be of much assistance, although it was utilized in the correction of errors. Attention was enforced on counting by focusing on auditory and throat sensations; on writing, by focusing auditory aspect of the vocal cue. The decrease in conflict as the auditory aspect of the writing-cue became pronounced was evidenced both introspectively and by the lowering of the time-readings. Under all conditions of attention there was a great increase in the time taken to write verse. The increase was greatest when attention was on the counting; least when on the writing.

II. Reading Mentally and Writing.

(Normal time of writing for day, March 6, 65.4s.)

A. *Attention Undirected.*

One trial.

(*a*) Some fluctuation of attention. When acute, attention was on the reading. There was, however, little strain; the experiment was interesting and exciting. There was loss of the feeling of strain in hand experienced in normal writing and the hand moved more rapidly than B had anticipated. B was, however, conscious of tactile sensations and the hand was felt to be covering more than the normal extent of space. Many of the words read, when reviewed, aroused no sense of familiarity.

(*b*) The writing-cue was auditory-vocal-motor, the auditory aspect being pronounced. In this way the writing-cue was distinguished from the vocal-motor reading process. The verse came by lines and phrases. No visual material was noticed in writing, except visual report for sign '&' which was then changed to word 'and.'

(*c*) Time required for writing, 61.2s. Number of words read, 39. Lapses: 4; substitution of word ('April' for 'leap-year'), 1; repetition of letter ('t,' written first as a capital, repeated as small letter), 1; omission of letter-parts ('u' and 'x'), 2.

B. *Attention on Reading.*

One trial.

(*a*) Writing more nearly automatic than in any preceding test, although there was some slight awareness of tactile and kinæsthetic report from hand. Attention was not intensely concentrated on reading. Although words were read in groups they did not convey much meaning. An auditory-vocal-motor process accompanied reading but dropped out at moments of greatest concentration.

(*b*) There was no distinct consciousness of any writing-cues, unless, perhaps, an auditory cue at the beginning of lines.

(*c*) Time required for writing, 61.7s. Number of words read, 86. Lapses: 3; repetition of letter-parts ('n'), 2; omission of letter-part ('m'), 1. Many of the letters are badly formed.

C. *Attention on Writing.*

One trial.

(*a*) Auditory-vocal-motor reading process which was synchronous with the auditory-vocal-motor writing-process, resulting in a 'verbal duet' in which the verse-words were intoned in a sing-song fashion and the words read given a circumflex intonation. The attention was enforced on writing by a doubling of the auditory-vocal-motor writing process (both lines and words separately were vocalized), and by concentration on the auditory report (scratch of pen) and concentration on the tactile report.

(*b*) The writing-cue was auditory-vocal-motor for first two lines before the writing or reading began, followed by same process for each word. The same method was used throughout verse.

(*c*) Time required for writing, 64.3s. Number of words read, 26. Lapses: 1; substitution of word ('February' substituted for 'leap-year').

Summary.—The comparative time-readings of I and II show that visual distraction introduced much less conflict than did articulatory. Although vocalization accompanied both reading and writing, B apparently achieved a synchronization of the two vocalization or auditory processes. The doubling of the auditory-vocal-motor process as writing-cue when attention was concentrated on writing should also be noticed. Under all conditions of attention the time-readings are below the normal for the day. The low time-reading, the great number of words read and the introspective report all point to a condition approaching automatism in the writing of verse, while attention was concentrated on the reading.

III. Reading Aloud and Writing.

(Normal time of writing for day, March 12, 66.8s.)

A. *Attention Undirected.*

Two trials.

(*a*) First trial. B broke down just after starting second line in the *middle* of the word '*the*.' He could give no report as to what had taken place; he did not know what he had written.

Second trial. The experiment was found to be difficult but exciting, becoming easier toward close. There was evident a conflict of the auditory-vocal-motor processes which accompanied both writing and reading. There was a persistent tendency to write the words and letters which were being read.

(*b*) First trial. No report of any writing-cue. Second trial. The writing-

cue was an auditory-vocal-motor process, although possibly this process was in the nature of a report rather than a cue. The report from writing was, in general, in terms of kinæsthetic sensations from arm and hand and auditory sensations from the scratching of the pen. This report, however, lapsed as soon as attention went to the reading.

(*c*) Time required for writing: first trial, no report; second trial, 104s. Number of words read: first trial, no report; second trial, 32. Lapses: first trial, 2; repetition of letters ('hass' for 'has,' 'Septembeber' for 'September'). Second trial, 8; repetitions of syllables, letters, or letter-parts, 4; omissions of letter and letter-part, 2; anticipation of letter, 1 ('Steptember' written for 'September' with tendency in the next line to repeat 'September' in place of 'November.' B wrote 'Se' and then crossed it out and went on with correct word); coalescence of word read with letter seen, 1 (word 'done' was written preceding the word 'days,' probably an auditory combination from 'd' and word 'tongue' which was being read or seen). B reported that he was aware of the errors at the moment of making them but lost memory of them immediately. There was no way of testing the truth of his report.

B. *Attention on Reading.*

One trial.

(*a*) Attention not on reading throughout. Most of the reading was done in the breaks between the lines of writing. The hand moved easily when the vowels in the words written and read were the same.

(*b*) The writing-cue was auditory-vocal-motor; this cue was not, however, required for all the words written.

(*c*) Time required for writing, 146.2s. Number of words read, 82. Lapses: 14; intrusion of word-parts from reading list, 3 ('Thak tharty' written for 'Thirty'—the word read was 'bank'; 'Jufe' written for 'June'—word read was 'life'; 'fgh' written for 'fine'—word read was either 'eight' or 'night'); omissions of letters in whole or part, 4; repetition of syllables or letter-parts, 6; repetition of word, 1; reversion in fourth line of verse to wording of verse learned originally.

C. *Attention on Writing.*

One trial.

(*a*) This test was much easier than the preceding. Attention was, however, not strongly concentrated on writing. There was little tendency to write the words read.

(*b*) The writing-cue was an auditory-vocal-motor process, with an emphasis of the auditory aspect, which marks it off from the writing-cue of the preceding test.

(*c*) Time required for writing, 75s. Number of words read, 26. Lapses: 7; omissions, 3 (two letters and one word); repetition of letters, 2; insertion of letter, 1; in writing the word 'June,' part of the 'u' was omitted and in attempting to straighten out the kinæsthetic report B. doubled the 'e.'

Summary.—A comparison of the results of III and II indicates that the introduction of overt articulation increased the difficulty of the test and raised the time-readings, a result com-

parable with that obtained in I. The corresponding series (A, B, C) in I and III are closely comparable. Attention upon the articulatory process, probably equivalent to auditory distraction, retards the writing; whereas attention upon the writing, through auditory enforcement, facilitates it. The number correspondence in words read in II and III (A, B, C) should be noticed.

The loss of the visual perceptual series in III is shown by the large number of lapses, in contrast to the result in I. Again, the attentional value of articulation is shown, in contrast to II, by the character of the lapses in III.

IV. Counting Aloud and Writing. Blindfolded.
(Normal time for day, March 19, 62s.)

A. *Attention Undirected.*[1]

One trial.

(*a*) Some oscillation of attention. After a word or phrase was started, it was allowed to run itself. The general mood was one of relaxation. There was a strong kinæsthetic report from the hand, which was reassuring; and, also, a blurred visual report which was disconcerting. The auditory report from the pen-scratch was also evident. B. was aware of errors but not in detail.

(*b*) The writing-cue was auditory-vocal-motor. At the beginning of one line the sweep of the arm from the close of the preceding line was felt to constitute the cue.

(*c*) Time required for writing, 94s. Number counted, 72. Lapses: 10; omission of letters or letter-parts, 3; repetition of letters or letter-parts, 6; doubtful error, 1.

B. *Attention on Counting.*

One trial.

(*a*) B counted in a low voice; attention was not intensely concentrated on counting. There was some kinæsthetic, some visual report from writing; in the first line there was a lapse of all report with accompanying uncertainty as to result.

(*b*) The writing-cue was auditory-vocal-motor, with more cues utilized than in the preceding test.

[1] This test was repeated May 21; and again May 28. The results closely parallel those of March 19. Time, May 21, 94s; May 28, 80s. Number counted, May 21, 71; May 28, 73. Lapses, May 21, 13, viz., omissions of letters or letter-parts, 7; repetition of words or letters, 6. May 28, 6, viz., repetitions, 3; exhange, 1; substitution, 2. In both tests some auditory-vocal-motor control was used, but there was much automatism. B reported that the lapses were due to the return of attention to writing and that a more perfect distraction would reduce errors.

(*c*) Time required for writing, 109s. Number counted, 123. Lapses: 14; repetition of letters and letter-parts, 8; omission of letter and letter-parts, 3; transposition of letter-part, 1; errors of substitution or anticipation, 4.

C. *Attention on Writing.*

One trial.

(*a*) Counting went smoothly while attention was on writing. Toward the close of the test attention was diffused. As a matter of fact over a third of the counting was done during the writing of the first line of verse.

(*b*) The writing-cue was auditory-vocal-motor, word by word. By the time B reached the middle of a word there was a lapse, or perhaps retardation of the kinæsthetic report, so that he was uncertain as to what he had written. This difficulty caused errors.

(*c*) Time required for writing, 112s. Number counted, 87. Lapses: 12; repetition of word- and letter-parts, 7; omission of letters and letter-parts, 3; transposition of letter-part, 1; repetition of substituted letter, 1.

Summary.—The writing-cue, which was auditory-vocal-motor, occurred less frequently in this series than heretofore. As compared with I, there was evident a growing automatism with reference to the writing-sequence. This result is probably evidenced by the lowering of the time-readings in IV as compared with I, although the absence in the latter case of visual distraction may also be influential. The large number of lapses was probably due to the fact that although writing tended to become automatic, attention returned to it at places and so introduced errors. B spoke at times of retardation of kinæsthetic report from the hand. As before, when there was articulatory distraction, there was a lengthening of the time needed for writing.

V. Writing while Counting Mentally the Number of Times that a Particular Word Occurred in a Rhyming List Read Aloud by the Experimenter.

(Normal time of writing verse for day, June 3, 61s.)

A. *No Directions as to Attention. Eyes Blindfolded.*

One trial.

(*a*) Distraction was perfect. Writing was much more automatic than ever before. There was little return of attention to movement.

(*b*) At long intervals an auditory writing-cue was obtained and there was a vague kinæsthetic report of writing very rapidly.

(*c*) Time required for writing, 65s. Lapses: 4.

General Summary.—Attention went to the distracting

rather than to the writing process. To focus attention on the motor side of the writing process, either the kinæsthetic report or the visual report, was to disorganize the movement. To intensify the writing-consciousness, B concentrated on the anticipatory side and emphasized the *auditory* aspect of the vocal-motor cue. At times there was a doubling of the vocal process. Such a method of intensification introduced conflicts with an articulatory distraction—conflicts partly resolved by the dissociation of auditory and articulatory imagery. Reading mentally caused less conflict than did articulation, and writing became more automatic but gave evidence of many lapses. There was a strong tendency to write the words being read. When distraction became *complete* in V, there was complete surrender to grapho-motor automatism and the time required for writing the verse approximated the normal. There were fewer lapses in V than in II, B, a test in which writing also approached automatism, due perhaps to the fact that the *auditory* appeal to attention was stronger than the visual appeal, and distraction consequently more *complete,* or that the verse sequence was more perfectly mastered at the time of the later test. B was the only reagent, except A, who succeeded in writing during the distraction of test V, a distraction which permitted little oscillation of attention. B's success in this test is evidence of the high degree to which his writing is automatised. The completeness of the distraction testifies to the appeal made to B's attention by the *auditory* factor. The result is perfectly characteristic of the subject.

Many lapses occurred in B's writing of the verse, due, he asserted, to the lapsing or retarding of the grapho-motor report. The sudden and unnecessary return of attention to the writing would occasion a repetition of the letters just written, of the writing of which B had had no report. Observation of the errors made show, further, that a letter was often omitted because of its graphic similarity to some letter just made; or a letter was split and its parts occurred separately—a purely motor lapse.

Distraction Experiments. Verse.

SUBJECT D.

(Time of writing verse after perfectly memorized, for ten trials, 74.3s.; M.V., 2.6s.)

I. Counting Aloud and Writing. Eyes Open.

(Normal time of writing for day, January 31, 74.3s. Normal rapidity of counting, 132 in 60s. One trial each for counting and writing.)

A. *Attention Undirected.*

One trial.

(*a*) The experiment was pleasant and exciting. It did not involve the effort noticed in reading and writing at the same time. There was little oscillation of attention; but, also, but very little meaning to writing. The sight of the result was reassuring and attention was concentrated mainly on the visual perceptual result.

(*b*) Writing was guided by visual perception of the result. D watched intently the words as they flowed from the pen. There was no consciousness of incipient articulation.

(*c*) Time required for writing, 73.2s. Number counted, 123. Lapses, 0; but the writing was nervous.

B. *Attention on Counting.*

One trial.

(*a*) D counted slowly with a raised and monotonous voice in the effort to force attention on the sound of the voice. It was difficult to keep attention on the counting. The visual perception of the writing was so distracting that D felt that she could follow directions better if the eyes were closed. (Cf. IV. B.)

(*b*) There was a little auditory-motor vocalization used as writing-cue. The chief cue, however, seemed to be the contact sensations. There was a strong tendency to look off the paper.

(*c*) Time required for writing, 68s. Number counted, 108. Lapses: 1, viz., one repetitionary loop.

C. *Attention on Writing.*

One trial.

(*a*) The counting was largely automatic with occasional jerks of attention in its direction. There was little meaning to either process.

(*b*) The writing-cue was auditory-vocal-motor with an intensification of the auditory aspect. Attention also followed assiduously the visual perceptual series.

(*c*) Time required for writing, 67.6s. Number counted, 131. Lapses: 0; but writing is highly nervous, movements were uncontrolled.

Summary.—The writing-control was largely visual-perceptual, a control by seen result of movement, not by anticipation. When attention was directed to a specific process it was enforced, for counting, by a voluntarily intensified auditory-sen-

sational series; for writing, by intense visual concentration on the letters as they were written and also by the appearance of a vocalization-cue with a strongly marked auditory aspect. D's report that writing was facilitated by counting was confirmed by the lowering of the time-readings.

II. Reading Mentally and Writing.

(Normal time of writing for January 17 and January 24, 78s. and 77s.)

A. *Attention Undirected.*

One trial (January 17).

(*a*) There was oscillation of attention but with intensification of the whole process. The experiment was pleasant, although there was a distressing sense that the hand-movement was inadequate. It felt irresponsible as to details. The eye dwelt on the reading-list, word by word, in attempt to get meaning into the words. The pull of the eye from list to writing was noticeable. D was also conscious of the hand in the periphery of vision.

(*b*) The writing-cue was auditory-vocalization. There were occasional glimpses of a visual report at places where the grapho-motor report seemed slip-shod. There was at places, perhaps, a complete lapse of kinæsthetic report from the hand.

(*c*) Time required for writing, 66.8s. Number of words read, 39. Lapses: 2; omission of letter, 1; repetition of letter-part, 1.

B. *Attention on Reading.*

Two trials (January 17 and January 24).

(*a*) First trial. D found the experiment difficult since attention went to the writing rather than to the reading. Attention was, probably, on the writing during a greater part of the time. D attempted to force attention on the reading by vocalizing the words read, but this introduced conflict with the writing-cues. D also attempted to force attention on reading by movements of the head and by narrowing the eyes. Second trial. D succeeded in obeying directions only for a few seconds at a time. She enforced the reading process by an emphatic vocalization. When attention returned to writing, the writing-cues were also auditory-vocal-motor.

(*b*) First trial. The writing-cue was auditory-vocal-motor. There was a conflict of the processes utilized in reading and writing. The auditory aspect of the writing process was more pronounced than that of the reading process. Second trial. The writing-cue was auditory-vocal-motor but with a more pronounced auditory aspect than the vocal reading-process.

(*c*) Time required for writing: first trial, 75.4s.; second trial, 77s. Number of words read: first trial, 44; second trial, 68. Lapses: first trial, 6; repetition of letter-parts, 3; omission of letter-parts, 2; doubtful, 1. Second trial, 4; repetition of letter-part, 1; substitution of word-part, 1; omission of a word, 1, (D left a blank space, being unable to recall word); writing of 'its' for 'it,' 1.

C. *Attention on Writing.*

One trial (January 24).

(*a*) Most of the words were vocalized although some were merely seen. Attention was kept on the writing by utilizing a visual report from writing. This report was very distinct. Attention was strongly visual. D bent low over the writing-table and there was a feeling of eye-strain.

(*b*) The writing-cue was auditory-vocal-motor. The words came in long series, probably reading ceased at these moments. The *visual report* was complete and distinct.

(*c*) Time required for writing, 83s. Number of words read, 33. Lapses: 1; omission of letter-part, 1.

Summary.—The writing-cues in this experiment were chiefly auditory-vocal-motor with a pronounced auditory aspect. Attention was enforced on reading by the intensification, probably, of the motor side of the mental vocalization; on writing, by concentration on a visual report. The latter method of enforcement retarded writing somewhat. Except in C there was frequent lapsing of kinæsthetic report from the hand. That the test is somewhat more difficult than I, where there was visual perception of results, is shown by the greater number of lapses. As in I, however, the distracting process intensified the whole experience and the time-readings were below, or approximated, the normal.

III. Reading Aloud and Writing.

(Normal time of writing for day, February 21, 73.4s.)

A. *Attention Undirected.*

One trial.

(*a*) The experiment required effort. There was some oscillation of attention. D read rapidly, almost automatically. Although there was little sense of meaning in going from line to line of writing, the writing did not seem automatic.

(*b*) The writing-cue was auditory-vocal-motor. There was, also, a certain amount of visual report which may have served as cue to some extent.

(*c*) Time required for writing, 74s. Number of words read, 80. Lapses: 4; omission of letter-part, 2; repetition of letter-part, 1; substitution of word, 1. D wrote only the first two letters of substituted word, then crossed them out and went on with the correct word.

B. *Attention on Reading.*

One trial.

(*a*) D read intently. At times the voice was very loud and its pitch was raised. Once or twice there was a tendency to pronounce out loud the words that were being written. One word was actually so spoken, but D was unaware of the fact that it belonged to the writing-sequence.

(*b*) The writing-cues were auditory-vocal-motor but there were fewer cues present than in A. Meaning-consciousness (?) was utilized at places in getting a cue.

(*c*) Time required for writing, 79.8s. Number of words read, 79. Lapses: 5; repetition of letter-part, 1; substitution of letter (from reading), 1; omission of letter, 1; transposition of letter, 1; anticipation of letter, 1, corrected.

C. *Attention on Writing.*

One trial.

(*a*) There was little fluctuation of attention. D read slowly and laboriously and yet automatically, the reading process seemed, in general, a more automatic one than the writing process. Attention went so naturally to writing that the conditions of the experiment brought the subject great relief.

(*b*) The writing-cue was auditory-vocal-motor with the visual report also utilized as cue.

(*c*) Time required for writing, 79.2s. Number of words read, 58. Lapses: 2; repetition of letter-part, 1; substitution of word 'hath' for 'have,' a substitution immediately corrected.

Summary.—As in II, the writing-cue was auditory-vocal-motor with a varying amount of visual report, probably also utilized as a control. In III as in II the direction of attention to either process retarded the process. Attention on reading was enforced by intensification of the auditory-motor sensations; attention on writing, by automatization of the reading process, which D reported as a peculiar experience and which results show to be a more effective method than that employed in II C. In contrast to II there was little record in III of lapsing of kinæsthetic report from the hand. In comparison with II reading was carried on much more rapidly, a rapidity due perhaps to the facilitation of the meaning-consciousness by overt articulation. The time-readings were slightly above normal.

IV. Counting Aloud and Writing. Blindfolded.

(Normal rapidity of writing for day, March 2, 71s.)

A. *Attention Undirected.*

One trial.

(*a*) Pleasant excitement. Counting eased the strain on the hand normally present. Counting was automatic up to 100. During the writing of the first four lines attention was largely on the writing; during the writing of the fifth and sixth lines it seemed to be diffused over counting and writing. Convulsive movements of the hand were noticeable in places.

(*b*) The principal writing control was the visual report from writing. At one or two places where D hesitated an auditory word-cue was used.

(*c*) Time required for writing, 67s. Number counted, 132. Lapses: 3; repetition of letter-part, 1; repetition of letter, 1; repetition of word, 1, this word was written, then crossed out and rewritten. In counting, 79, 89 and 99 were omitted.

B. *Attention on Counting.*

One trial.

(*a*) Very hard to keep attention on counting. D counted slowly and in a loud labored voice. There was also a faint visual accompaniment to counting. Breathing was labored; head thrown back; eyes frequently not focused on paper.

(*b*) There was much hesitation in writing. As control, auditory-motor vocalization was used frequently; visual report was also utilized. There was, however, less visual report than in A.

(*c*) Time required for writing, 85s. Number counted, 107. Lapses: 2; repetition of letter-part, 1; omission of letter-part, 1.

C. *Attention on Writing.*

One trial.

(*a*) D bent very low over the table concentrating attention on the visual report from the writing, which was very fine and even. Down to the fourth line attention was thoroughly occupied with the writing. Up to this point D counted in a low-pitched subdued even voice. At the fourth line D's attention wandered from the writing, due to speculation as to the reason for omitting certain numerals in counting in B. At this point the voice became louder and more labored and the visual report from writing lapsed somewhat.

(*b*) There were auditory-vocal-motor cues throughout; the lines were said as wholes. Attention, however, was on the visual report which was very distinct and seemed effective as control.

(*c*) Time required for writing, 62.8s. Number counted, 117. Lapses: 3; repetition of letter-part, 1; omission of letter-part, 1; omission of letter, 1.

Summary.—The results in IV are closely comparable to those of I. In both cases, counting, which is automatic, facilitates writing. The one exception to this statement, IV B, should be compared with I B. Apparently the ideational visual control was less automatic than the perceptual visual control. In both tests visual material is prominent.

V. Writing while Counting Mentally the Number of Times that a Particular Word Occurred in a Rhyming List Read Aloud by the Experimenter.

(Normal time of writing for day, August 3, 71s.)

A. *Eyes Open.*

Three trials.

(*a*) First trial. Failure. D broke after first word of second line. Second trial. D broke at end of third line. Third trial. D wrote all of verse but last word; but after counting thirty-one occurrences of the test-word, she simply kept repeating 'forty-one' every time the test-word occurred.

(*b*) Control was largely perceptual and visual.

(*c*) Time required for writing: second trial, 41s; third trial, 88s.

B. *Eyes Closed.*

Three trials. Failures. Impossible to maintain writing under the conditions of the test.

General Summary.—Attention went normally to writing rather than to the distracting processes. Both reading and counting proved to be more automatic than writing. Counting, in fact, was so automatic as not to be a distraction at all; indeed, by release of nervous energy, it served, rather, to facilitate writing. To keep attention on counting, D intensified vocal sensations or attempted to visualize numbers. No effort was needed to hold attention on writing, but in intensification of such attention, D concentrated intensely on the visual report, either sensational or ideational, from writing. This is really a mode of concentration on the *movement-aspect* of the situation. Visual distraction was a more effective mode of distraction than articulation and sometimes disturbed the consciousness of the kinæsthetic report from the writing, a disturbance of which the reagent was distressingly aware. There was noticed a much stronger tendency to say aloud what was being written than to write what was being read (cf. A). On the whole, articulatory automatism was evident. D is aware of a tendency to such automatism in other situations likewise.

Lapses occurred rarely. When they were made, D was usually conscious of their presence and of their nature.

Distraction Experiments. Verse.

SUBJECT S.

(Average time of writing verse, after it had been perfectly memorized (eight trials), 73.24s; M.V., 1.87s.)

I. Counting Aloud and Writing. Eyes Open.

(Normal time of writing for day, February 16, 72.4s.)

A. *Attention Undirected.*

One trial.

(*a*) Experiment was difficult. Attention oscillated deliberately. There were three breaks in writing, that is, points of distinct hesitation when S found it hard to get the next word.

(*b*) The writing-cue was visual and anticipatory. This cue was necessary at the beginning of lines and in the case of difficult words. When the breaks

occurred in the writing, S was obliged to have recourse to visual perception of what had already been written, a process followed by obtaining an anticipatory visual cue of what was next to be written. At times, S noticed a tendency to articulate words of verse rather than the numbers but in general there was no consciousness of articulation of verse.

(*c*) Time required for writing, 182s. Number counted, 209. Lapses: 0.

B. *Attention on Counting.*

Two trials.

(*a*) First trial. S broke at end of third line. Counting was judged *not* to be automatic. Second trial. Attention was largely on counting, although between the counts attention went to the writing. Attention was enforced for the counting by listening to the sound of the voice.

(*b*) Anticipatory visual images occurred as writing-cues at difficult places, but in general they occurred less frequently than in I A. Writing control was more definitely a sensory control in terms of the grapho-motor report.

(*c*) Time required for writing: first trial (three lines), 74s.; second trial (whole verse), 127.8s. Number counted: first trial, 131; second trial, 189. Lapses: 2; repetition of letter, 1, corrected; omission of word, 1, not perceived.

C. *Attention on Writing.*

One trial.

(*a*) Oscillation of attention, for instance, at the beginning of every line where S stopped counting in order to get an anticipatory visual image. Attention throughout test was strongly visual; eyes were focused on paper.

(*b*) Many visual images were used as cues; more were in evidence than in I B. S forced attention to writing by concentrating on sensational and ideational visual processes.

(*c*) Time required for writing, 112s. Number counted, 179. Lapses: 3; repetition of letters, 2; anticipation of letter, 1, corrected. A transposition occurred in the counting.

Summary.—The test shows S's dependence upon visual processes, both sensational and ideational. These processes were utilized in enforcing attention and at points of difficulty. Although no conflict of control processes for writing and counting was reported, the effort experienced in focusing attention was evident throughout, as shown by the high time-readings.

II. Reading Mentally[1] and Writing.

(Normal time of writing for day, February 21, 77.6s.)

A. *Attention Undirected.*

One trial.

(*a*) Oscillation of attention throughout. S read about six words at a time,

[1] In general, S does not articulate in reading; but sometimes reads out loud in order better to concentrate on what he is reading.

then articulated the words of the verse. There was little articulation of the words read. S was strongly conscious of his hand and of a tendency of the eyes to follow attention. The eyes fell to the writing frequently, particularly at the beginning of lines. S also looked down when in doubt as to the next letter.

(*b*) The writing-cue was incipient articulation. No visual imagery occurred in the form either of anticipation or report.

(*c*) Time required for writing, 104.4s. Number of words read, 52. Lapses: 1; omission of letter, 1.

B. *Attention on Reading.*

One trial.

(*a*) No particular method was used to enforce attention on reading. Attention went continually to writing, from which it was brought back forcibly. There was a strong tendency for eyes to drop to paper.

(*b*) Articulation of words of verse served as the writing-cue. S tried to inhibit this articulation but could not.

(*c*) Time required for writing, 92s. Number of words read, 58. Lapses: 2; anticipation of letter, 1; substitution of word, 1, corrected.

C. *Attention on Writing.*

One trial.

(*a*) Attention forced to writing by double visual series; one, an anticipatory; the other, a report series. The visual cues and report interfered with the perception of the words being read. S, however, kept his eyes on each word until he got its meaning. There was no tendency to 'lip-motor' reading, but there was a strong tendency to drop eyes to the writing.

(*b*) The writing-cue was visual anticipation accompanied by a certain amount of articulation. The writing was also reported visually.

(*c*) Time required for writing, 92.5s. Number of words read, 37. Lapses: 1, corrected.

Summary.—In II as compared with I there was a shift in writing-control from visual to articulatory, although the visual control returned with the focusing of attention upon writing. A comparison of the time-readings for I and II shows that articulation as a distraction introduced greater disturbance than did visual distraction.

III. Reading Aloud and Writing.

(Normal time of writing for day, April 25, 74s.)

A. *Attention Undirected.*

One trial.

(*a*) Oscillation of attention. There was no tendency to write the words that were being read.

(*b*) The writing-cue was, chiefly, incipient articulation which possessed no auditory aspect. Some visual imagery was utilized as cue, but not a great deal. There was no visual report.

(*c*) Time required for writing, 160s. Number of words read, 95. Lapses: 9; repetition of letter-part, 2; repetition of word-part, 1; repetition of letter, 1; repetition of word, 1, S. was aware of this error; omission of letter-part, 3; transposition of letter-part, 1.

B. *Attention on Reading.*

One trial.

(*a*) Oscillation of attention. The act of reading calls for attention; overt articulation demands effort. No tendency to write words being read, but distinct tendency to articulate words being written.

(*b*) Either an articulatory or a visual cue necessary for every word written. There were fewer articulatory and more visual cues than in the preceding experiment. The grapho-motor report was much less prominent than it is in normal writing. S recalled no errors.

(*c*) Time required for writing, 127.5s. Number of words read, 73. Lapses: 9; repetition of letter-part, 5; repetition of letters, 1; omission of letter-part, 2; omission of letter, 1.

C. *Attention on Writing.*

One trial.

(*a*) Oscillation of attention; writing maintained at the expense of reading.

(*b*) More visual material than before;[4] it took the form both of cues and report. S was aware of no errors and had very little consciousness of his hand.

(*c*) Time required for writing, 106s. Number of words read, 49. Lapses: 2; repetition of letter-part, 1; repetition of letter, 1.

Summary.—With the return to an articulatory distraction, there was a gain in the amount of visual material over that reported in II (visual distraction) and also a retardation in speed. Articulatory and visual distraction together are scarcely more difficult than articulatory distraction alone. The double distraction produced, however, a larger number of lapses than was found in II.

IV. Counting Aloud and Writing. Blindfolded.

(Normal rapidity for day, March 7, 72.4s.)

A. *Attention Undirected.*

One trial.

(*a*) S broke down in the middle of the fifth line in the *middle* of a *word* and was unable to recover himself although counting was continued for several seconds after the break.

(*b*) There was a visual cue, clearly anticipatory, at the beginning of each line, varying from whole to one or two letters of the first word. There was also some articulation of words between counts, although not every word was articulated. At places the writing approached automatism.

[4] S thinks that repetition of experiment under any conditions of attention would show increase in visual material. The method is workable.

(*c*) Time required for writing, 103.2s. Number counted, 146. Lapses: 1; omission of letter-part, 1.

B. *Attention on Counting.*

One trial.

(*a*) S tried to enforce attention on counting by listening to the sound of his own voice. He was not able to keep attention on counting throughout the test.

(*b*) Visual cues occurred rarely. When there was hesitation S. was conscious of articulatory cue. This articulation produced no tension in throat muscles and had no auditory aspect.

(*c*) Time required for writing, 109.7s. Number counted, 153. Lapses: 1; omission of letter-part, 1.

C. *Attention on Writing.*

One trial.

(*a*) Attention was not concentrated wholly on writing. It was difficult to keep attention from reverting to counting, for the latter process is not wholly automatic.

(*b*) During the writing of the first two lines S made frequent use of visual cues without articulation. He broke temporarily at the close of the second line and was obliged to go back and articulate these lines before going on with the writing. From this point on S made use of articulatory writing-cue.

(*c*) Time required for writing, 120.4s. Number counted, 158. Lapses: 2; repetition of letter-part, 1; substitution of word, 1, corrected immediately.

Summary.—As before, articulatory distraction caused difficulties. With the loss of the visual perception of the result, there was an increase in the number of vocal-motor cues, but on the whole the methods of control employed in I and IV are similar.

V. Writing while Counting Mentally the Number of Times that a Particular Word Occurred in a Rhyming List which was Read Aloud by the Experimenter.

(Normal rapidity for day, July 27, 71.2s.)

A. *Eyes Open.*

One trial (failure).

(*a*) S broke at the end of the second line. There was conflict of the vocal-motor processes used for writing-cue and for counting rhymes.

(*b*) A visual ideational cue at the beginning of every word; then grapho-motor control.

(*c*) Time required for writing, 45s. (two lines). Lapses: 0.

B. *Eyes Closed.*

Two trials (failures).

(*a*) First trial. S broke at end of third line. Second trial. S broke in middle of third line. Articulatory conflict.

(*b*) Writing-cue was articulatory in both trials.

(*c*) Time required for writing: first trial, 48.2s. (three lines); second trial, 51.2s. (two and a half lines). Lapses: first trial, 2; second trial, 2.

General Summary.—Attention went to writing rather than to the distracting process, when the distracting process was visual. When the distraction was articulatory, there was deliberate oscillation of attention. Counting was not at all automatic. Throughout the tests, much visual material, both anticipatory and report processes, was used. In intensification of writing-consciousness S made use of both visual anticipation and visual report. In II C this visual material conflicted somewhat with the reading. In II A there is some evidence of a 'shunting' of control from visual to vocal-motor. There are few lapses recorded. (The results obtained from S should be compared with those obtained from Y; but writing is much better automatized for S than for Y. Neither shows much vocal-motor automatism.)

Distraction Experiments. Verse.
Subject Y.

(Average time of writing verse, after it had been perfectly memorized (ten trials), 78.51s.; M.V., 3.014.)

I. Counting Aloud and Writing. Eyes Open.

(Normal time of writing for day, January 22 and January 29, 80.4s. and 78s. Normal rapidity of counting, 124 in 60s., January 22, and 125 in 60s., January 29. One trial each for writing and counting.)

A. *Attention Undirected.*

One trial (January 22).

(*a*) Marked oscillation of attention. Visual perception of word being written distracted attention from counting. Attention chiefly on writing. Conflict of visual imagery accompanying vocalization[8] of verse and counting.

(*b*) Writing-cue was vocal-motor and visual-vocal.

(*c*) Time required for writing, 165s. Number counted, 121. Lapses: 3; repetition of one letter, 1; anticipation of letter, 2. All the lapses were corrected.

B. *Attention on Counting.*

One trial (January 29).

(*a*) Marked oscillation of attention; tendency for attention to follow visual perception. Y found the experiment very difficult, due, he reports, to the fact

[8] This imagery was apparently the visualization of the position of the vocal organs.

that the sight of the writing distracted attention from counting (but cf. IV) and due also to articulation conflict. The words of the verse were articulated usually between the counting intervals.

(*b*) Writing-cue articulatory.

(*c*) Time required for writing, 178s. Number counted, 160. Lapses: 1; substitution of word, 1. In the counting Y. jumped from 130 to 140.

C. *Attention on Writing.*

One trial (January 29).

(*a*) Y wrote the first letter of the verse before he began to count. Counting was not at all automatic for him. There was a strong tendency to articulate aloud the verse-words. There were frequent conscious blanks in which he failed to get the needed word.

(*b*) Control was largely articulatory, although there was present at intervals anticipatory visual imagery.

(*c*) Time required for writing, 130.4s. Number counted, 119. Lapses: 4; repetition of letter-part, 1; anticipation of letter, 3, corrected. Many mistakes occurred in counting, most of which were corrected. Y., however, jumped from 87 to 89 and from 97 to 197 without noticing the mistake.

Summary.—Y's writing control in this experiment was articulatory and also, probably, as shown by the test on the phrase and name, grapho-motor, the latter being reported in both sensational and ideational terms. The disturbance introduced by the articulatory distraction resulted, apparently, in part from a conflict in processes, but in part also from a reluctance to allow processes to proceed without supervision. Lip-innervation seemed to interfere with hand-innervation. The shifting of attention was a labored process.[6]

II. Reading Mentally and Writing.

(Normal time of writing for day, February 6, 78s.)

A. *Attention Undirected.*

One trial.

(*a*) Attention oscillated. Y read only at intervals. Reading at times was merely visual; at other times auditory and articulatory as well as visual. Attention went naturally to writing, Y being more than normally conscious of what was being written. The hand and the writing-movement fell in the peripheral field of vision and there was a strong tendency for the eyes to fall to the writing. There was a conscious attempt to split field of vision into central and peripheral portions.

(*b*) Writing-cue not reported.

(*c*) Time required for writing, 101.4s. Number of words read, 39. Lapses: 4; repetition of letter-part, 2; omission of letter-part, 1; omission of letter, 1.

[6] Y reports, in general, considerable inertia in mental processes.

B. *Attention on Reading.*

One trial.

(*a*) Whenever attention was really on the reading there was a tendency to write the words that were being read. Attention was kept on the reading by articulation of words read; at times this articulation was audible.

(*b*) The writing-cue was articulatory.

(*c*) Time required for writing, 146.6s. Number of words read, 39. Lapses: 8; repetition of letter-part, 3;[1] repetition of letter, 1; repetition of word, 1; omission of letter-part, 2; omission of letter, 1.

C. *Attention on Writing.*

One trial.

(*a*) Oscillation of attention, although the words were read visually.

(*b*) The writing-cue for the first three lines of the verse was articulatory; for the last three lines a not wholly successful attempt was made to throw attention on hand-movement (grapho-motor report) and use this as control. There was a visual report in several places but this report was thought *not* to serve as *cue*. Attention was on the hand-kinæsthetic report; each movement a cue for the next one.

(*c*) Time required for writing, 103.8s. Number of words read, 10. Lapses: 2; repetition of letter-part, 2.

Summary.—In general, II as compared with I shows, as evidenced by the time-readings, less disturbance from visual than from articulatory distraction, a result due perhaps to absence of conflict of articulatory processes; when this conflict returns, in II B there is a noticeable increase in the time of writing. Effort to attend and rely upon the hand-kinæsthetic series is more apparent in II than in I.

III. Reading Aloud and Writing.

(Normal time of writing for days of experiment, February 20 and February 27, 73s. and 82.2s.)

A. *Attention Undirected.*

Two trials.

(*a*) First trial. Y read very calmly and deliberately; no 'flickering' of attention. Second trial. Y tried to keep his attention on reading. When meaning was got from the words read there was a strong tendency to write the words.

(*b*) First trial. Y articulated every word written; in some words there was throat-; in some, tongue-; and in others, lip-innervation. This articulation had little auditory accompaniment. Visual report from the writing made

[1] This repetition consisted each time of an extra stroke on an 'm.' Y reported that in general there is need of a faint visual cue if he is to write well. When, as in the case of an 'm,' the hand-movement outruns the visual control, the movement is poorly coördinated; the loss of control is sometimes apparent in the following syllable likewise.

evident that this cue was inadequate. There were no anticipatory visual cues. Second trial. Throat-innervation for every word written but hand did not follow this cue accurately. Errors were reported in kinæsthetic terms supplemented by a visual report. Y voluntarily allowed the question of letter-formation to drop out of consciousness.

(*c*) First trial. Time required for writing, 168s. Number of words read, 44. Lapses: 14; repetition of letter-part, 3; repetition of letter, 6; repetition of syllable, 1; omission of letter, 2; exchange of letter, 1; substitution of word, 1, corrected. Letters were so badly formed that it was hard to determine the nature of the error. Y was aware of some of the errors through the visual or grapho-motor report, although the report was not detailed.

Second trial. Time required for writing, 148s. Number of words read, 63. Lapses: 13; repetition of letter-part, 6; repetition of letter, 2; omission of letter-part, 2; omission of word, 2; exchange of letter, 1; substitution of letter, 1. Letters were so badly formed as to make classification of errors difficult.

B. *Attention on Reading.*

Two trials (February 20 and February 27).

(*a*) First trial. Y read more rapidly and spasmodically than in A, particularly in the first part of the verse. Attempt was made to keep attention on reading by articulation of each word as soon as seen. Second trial. Y was tired at time of experiment. The test demanded great effort and left Y exhausted. There was great muscular strain experienced throughout test. Attention oscillated between writing and reading with no synchronizing of processes. It took distinct effort to complete the writing of words even after they had been once started.

(*b*) First trial. The writing-cue was the articulation of every word and in some cases of the letters also. Second trial. Distinct articulation of every word. Some errors were reported visually.

(*c*) First trial. Time required for writing, 155s. Number of words read, 70. Lapses: 16; repetition of letter-part, 7; repetition of letter, 2; omission of letter-part, 1; omission of letter, 4; omission of word, 1; anticipation of letter (letter split), 1. Second trial. Time required for writing, 140s. Number of words read, 80. Lapses: many of the words end in a meaningless scrawl. One whole line, the second, is omitted; so far as estimation is possible there are 16 lapses, viz., repetition of letter-part 1; repetition of letter, 1; omission of letter-part, 1; omission of letter, 3; omission of syllable, 1; omission of words, 5; anticipation of letter, 2; unclassified, 2.

C. *Attention on Writing.*

One trial (February 27).

(*a*) The attention was actually on the reading, not the writing. The experiment was disagreeable and very fatiguing. The whole coördination was broken by inhibiting the tendency to write the word that was being read.

(*b*) The writing-cue was throat-articulation; sometimes, actual lip-articulation.

(*c*) Time required for writing, 158.2s. Number of words read, 70. Lapses: 8; repetition of letter-part, 2; repetition of letter, 2; omission of letter-part, 1; omission of letter, 1; omission of word, 2.

Summary.—These experiments were so fatiguing and disagreeable to Y that several tests were omitted. There is evident, as in I, conflict of articulatory processes, with an accompanying increase in time-readings. But in comparison with I, the loss of visual perceptual guidance is evident, as shown by the increased number of lapses. Y attempted to allow the hand-movements to run themselves in III, in contrast to II, with the result that the coördinations went badly adrift, as was evidenced both by the lapses and the malformation of letters. The increase in the number of words read in III as compared with II shows again the bid made by the articulatory process upon attention.

IV. Counting Aloud and Writing. Blindfolded.

(Normal rapidity for day, March 7, 73.6s.)

A. *Attention Undirected.*

One trial.

(*a*) There was oscillation and dispersion of attention. When attention left the writing there was a tendency to write the word that was being pronounced.

(*b*) The writing-cue was articulatory, word- and letter-cue, but the cues were less prominent than in the normal writing for the day. At times, there was a visual report from the writing. Intrusion of meaning-consciousness was annoying.

(*c*) Time required for writing, 163s. Number counted, 86. Lapses: 11; repetition of letter-part, 3; omission of letter-part, 3; repetition of letter, 4; anticipation of word, 1, immediately corrected. Y. was also aware through the visual report of the repetition of one letter.

B. *Attention on Counting.*

One trial.

(*a*) Y counted six before beginning to write; he counted more rapidly than before; attention a great part of the time was not on the counting at all.

(*b*) Very little articulation was used as writing-cue except when there was difficulty in getting a word started. The cue was an anticipatory visual cue of hand, pen, and word to be written. Many of the lapses were reported. When report was in grapho-motor terms and 'fixed visually' Y was able to specify error. Otherwise he was only aware that a mistake had been made.

(*c*) Time required for writing, 178.8s. Number counted, 120. Lapses: 10; repetition of letter-part, 3; repetition of letter or letters, 3; repetition and anticipation of letter, 1; omission of letter-part, 1; omission of letters, 2.

C. *Attention on Writing. No Test.*

Summary.—IV shows more anticipatory visual material than do the other distraction tests. It is closely comparable

with I, the ideational visual material supplying to a certain extent the control furnished perceptually in I. The former control is, however, less efficient than the latter as is evidenced by the greater number of lapses in IV. The time-readings in both cases are high.

V. Writing while Counting Mentally the Number of Times that a Particular Word Occurred in a Rhyming List Read Aloud by the Experimenter.

(Normal time of writing for day, August 2, 73s.)

A. *Eyes Open.*

Two trials.

(*a*) First trial. Failure. Y broke completely at the end of the second line. Mental articulation used in counting words conflicted with the vocal-motor cue for writing. ' Counting could not go on visually although the results from the counting were so held. Second trial. Y broke at end of first line. The visual perception of the writing distracted attention from counting.

(*b*) First trial. The writing-cue was articulatory. Second trial, no report.

(*c*) Time required for writing. First trial (two lines), 40s. Second trial (one line), no record. Lapses: first trial, one repetition of letter-part. Second trial, omission of two letters.

B. *Eyes Closed.*

Two trials (failures).

(*a*) First trial. Y broke after second word. Second trial. Y broke in the middle of second line.

(*b*) Writing-cue, second trial, was articulatory. There was also a visual report from writing which conflicted with the visualizing of the numbers.

(*c*) Time required for writing, second trial (one and a half lines), 40s. Lapses: 7; repetition of letter, 3; omission of letter, 4; anticipation of letter, 1.

General Summary.—Attention went to writing rather than to the distracting process when the distracting process was visual. When the distracting process included articulation there was great conflict. Hand and vocal innervation seemed to inhibit each other. Counting was not at all automatic; the nearest approach to automatism was found in mental reading; the test involving this distraction is slightly less difficult than the other tests. Part of the difficulty for Y in the articulatory conflict was due to the use of a small verbal unit, usually the word or letter. In intensifying writing consciousness, attention was focused on the report of hand-movement and this movement was at times changed to a finger-movement in order to enforce consciousness. When attention was actually

on reading, there was a tendency to write the words read; at other times a tendency to say aloud the words being written.

Y's results should be compared with those of S; but Y found the experiment even more difficult than did S; writing is for him even less automatic than for the latter.

Y's record shows a very great number of lapses (80); this again is in contrast to the record of S.

Distraction Experiments. Writing of Verse.

SUBJECT R.

(Average time of writing verse, after it had been perfectly memorized (ten trials), 70.34s.; M.V., 2.644s.)

I. Counting Aloud and Writing. Eyes Open.

(Normal time of writing for day, November 13, 71s. Normal rapidity of counting, 153 in 60s. One trial each for writing and counting.)

A. *Attention Undirected.*

One trial.

(*a*) Attention flickered rapidly, returning to the verse at the beginning of lines. The conflict was greatest where numerals occurred in both processes. In the fourth line of writing the words to be written were interpolated between the separate counts. During the writing of the sixth line consciousness of writing lapsed at intervals.

(*b*) The writing-cue was probably auditory-vocal-motor.

(*c*) Time required for writing, 73.8s. Number counted, 133. Lapses: 3; omission of word, 1; substitution and repetition, 1; R started to write 'September' for 'November' but corrected the error; reversion to another wording of verse in the sixth line, 1.

B. *Attention on Counting.*

One trial.

(*a*) R counted in a louder voice than in A. Attention so far as it was focused on counting was concentrated on the sound of the voice. There was some oscillation of attention; but in the writing of the fifth and sixth lines attention was on the writing.

(*b*) The writing-cue in the first two lines was the grapho-motor sensational series. In the following lines auditory-vocalization was used at the beginning of lines and at difficult places.

(*c*) Time required for writing, 72.6s. Number counted, 148. Lapses: 3; omission of letter-parts, 2; omission of letter, 1.

C. *Attention on Writing.*

One trial.

(*a*) The test required more effort than did A. R counted in a low voice.

(*b*) The writing-cue was auditory-vocal-motor, word by word.

(*c*) Time required for writing, 78.5s. Number counted, 109. Lapses: 2; omission of one letter-part, 1; substitution of word, 1, corrected.

Summary.—Writing was so well organized in grapho-motor terms that the introduction of the counting process caused little disturbance, although there was considerable fluctuation of attention. Some auditory-vocal-motor cues were needed at intervals in writing the verse, and visual attention was utilized throughout. Attention on counting was enforced by intensification of auditory sensations; attention was focused on the sound of the voice. It required effort to give attention to writing and in doing so, R used more auditory-vocal-motor cues than under other conditions.

II. Reading Mentally and Writing.

(Normal rapidity of writing for day, November 8, not obtained.)

A. *Attention Undirected.*

One trial.

(*a*) There was some fluctuation of attention. The words read were vocalized mentally. Attention was on the writing at the points where an auditory-vocal-motor cue was found necessary, or at places where the hand-kinæsthetic report gave notice of an error.

(*b*) The writing-cue was largely automatic, a surrender to the grapho-motor sensational series. Auditory-vocal-motor cues were needed to get the lines started.

(*c*) Time required for writing, 80s. Number of words read, not reported. Lapses: 3; omission of word, 1; intrusion of letter, 1, immediately corrected; writing of small for capital letter, 1, reported kinæsthetically.

B. *Attention on Reading.*

One trial.

(*a*) Some oscillation of attention. Attention was kept on reading by mentally pronouncing every word. When attention went to the writing it was centered on the hand-movement. There was no visual material.

(*b*) No writing-cues were reported. The hand-kinæsthetic sensations were directive.

(*c*) Time required for writing, 68s. Number of words read, not reported. Lapses: 1; omission of a word.[1]

C. *Attention on Writing.*

One trial.

(*a*) Required even less effort than B.

(*b*) Auditory-vocal-motor cues were used throughout. The first line was spelled out letter by letter; the other lines were syllabified. There was little

[1] Omission of this word was a frequent error.

visualization, except in the last line where a small letter was incorrectly written for a capital one. This error was reported visually, and the report was followed by a visual image of the correct letter.

(*c*) Time required for writing, 66s. Number of words read, not reported. Lapses: 3; repetition of letter-part, 1; omission of word, 1; writing of small for capital letter, 1, corrected.

Summary.—This test like I shows a well-developed writing-automatism. Attention was maintained on reading by mental vocalization of the words read, and on writing by the same method. Very little visual material was present in writing. The time-readings for the writing of the verse are even lower than those in I, indicating probably that visual distraction caused even less conflict than did articulatory distraction. The relatively high time-reading in A was, perhaps, due to the novelty of the experiment.

III. Reading Aloud and Writing.

(Normal time of writing for day, November 13, 71.4s.)

A. *Attention Undirected.*

One trial.

(*a*) The experiment required effort. Attention flickered.

(*b*) An auditory-vocal-motor writing-cue was used frequently.

(*c*) Time required for writing, 84s. Number of words read, no record. Lapses: 3; omission of letters, 2; omission of letter-part, 1; R. was conscious of these mistakes but could not avoid making them. In reading the list several words were omitted.

B. *Attention on Reading.*

One trial.

(*a*) R found it difficult to keep attention on the reading. Several words were omitted from the reading-list.

(*b*) An auditory-vocal-motor writing-cue was used frequently.

(*c*) Time required for writing, 80.4s. Number of words read, not reported. Lapses, 4; omission of letter-part, 1; omission of letter, 1; repetition of letter-part, 1; substitution of letter, 1.

C. *Attention on Writing.*

One trial.

(*a*) R found the experiment difficult. In reading the list words written were interpolated, while words that belonged in the list were dropped from it. There was much hesitation in reading and little meaning in what was read.

(*b*) R used an auditory-vocal-motor cue, word by word. At one place she glanced down at paper.

(*c*) Time required for writing, 85.4s. Number of words read, no record. Lapses: 6; repetition of word, 1; of letter, 1; omission of letter, 2; substitution of letter, 1; reversion to another wording of verse.

Summary.—Overt articulation of the words read caused difficulties and raised the time-readings. As before, attention was concentrated on writing by slow vocalization.

IV. Counting Aloud and Writing. Blindfolded.

(Normal time for day, November 19, 71.3s.)

A. *Attention Undirected.*

One trial.

(*a*) Attention flickered rapidly from writing to counting. R lost count once but corrected her mistake.

(*b*) The words written were vocalized mentally. There was also a little visualization used as cue at the beginnings of lines. In one place the articulation of the verse-words was audible.

(*c*) Time required for writing, 78s. Number counted, 139. Lapses: 2; omission of word, 1; repetition of letter, 1; R. reported this error of which she became conscious, first through the grapho-motor, then through the visual report.

B. *Attention on Counting.*

One trial.

(*a*) R counted very rapidly and reported that the rapidity of the counting process hastened the writing for the two processes were rhythmized. There was less oscillation of attention than before.

(*b*) A few auditory-vocal-motor cues were used at intervals.

(*c*) Time required for writing, 64.4s. Number counted, 151. Lapses: 6; repetition of letter-part, 1; omission of letter, 2; omission of word, 2; substitution of letter, 1. Writing was nervous.

C. *Attention on Writing.*

One trial.

(*a*) The attention was concentrated on the hand and its movements. R more aware of kinæsthetic sensations from the hand than ever before.

(*b*) The control was consciously grapho-motor. There was little articulation of the verse.

(*c*) Time required for writing, 82.2s. Number counted, 150. Lapses: 3; repetition of letter-part, 2; repetition of letter, 1.

Summary.—As in the other tests there was evidence of articulatory conflicts. In focusing attention on the writing, R in this experiment attempted to concentrate on the hand and its movements, a method which retarded writing although reducing articulatory conflict. As compared with I results from IV evidence the loss of visual perceptual control.

V. Writing while Counting Mentally the Number of Times that a Particular Word Occurred in a Rhyming List that was Read Aloud by Experimenter.

(Normal time of writing for day, August 3, 63s.)

A. *Eyes Open.*

One trial (failure).

(*a*) Very difficult. Break at beginning of third line where R. returned to wording of first line.

(*b*) Surrender to grapho-motor sensations but with certain number of anticipatory vocal-motor cues.

(*c*) Time required for writing (two lines and a half), 45s. Lapses, a return to first words of first line.

General Summary.—Attention went to the distracting process rather than to writing. To focus attention on the actual writing-movement as in IV C retarded writing. Usually in intensifying the writing-consciousness R made use of slow vocalization, word by word, or even letter by letter. Such a method of intensification introduced slight conflicts with articulation, so that mental reading as a distraction caused less conflict than did the other methods of distraction employed. On the whole, surrender to grapho-motor automatism was an adequate way of meeting distraction. There were few errors made in writing. Part of R's success in dealing with the distractions was due to a wonderfully rapid oscillation of attention from process to process.

Distraction Experiments. Writing of Verse.

SUBJECT A.

(Average time of writing verse, after it had been perfectly memorized (11 trials), 56.8s.; M.V., 3.91s.)

I. Counting Aloud and Writing.

(Normal time for day, November 21, 57s.)

A. *Attention Undirected.*

One trial.

(*a*) Experiment required effort and was slightly disagreeable. A counted in a very low voice and wrote with many hesitations. Up to 100 counting was automatic at intervals; from 100 on not at all automatic. Throughout the experiment there was oscillation of attention; but more attention was given to writing during the first part of test.

(*b*) The writing-cue was auditory-vocal-motor with sensational visual control also evident. There was no ideational visual control. Twice there occurred a vague hand-motor cue, a partly anticipatory, partly sensational feeling of cramp in hand. This 'feeling' was attached to words difficult to write.

(*c*) Time required for writing, 113s. Number counted, 169. Lapses: 2; letter introduced in anticipation, 1, corrected immediately when perceived visually; omission of word ('it'), 1.

B. *Attention on Counting.*

One trial.

(*a*) A counted in a low voice. Counting-attention was enforced by ryhthmic grouping in 10's. After a group was once started the continuance was automatic. After 100 each word constituted a group. At the beginning of the experiment there was attention to throat and lips movement with anticipatory auditory-vocal-motor image of counting. Later, attention went to the auditory sensation arising from sound of counting.

(*b*) Writing-cue was auditory-vocal-motor with perceptual visual attention, although the latter was intermittent. The verse was written in the interstices of mind-wanderings from counting.

(*c*) Time required for writing, 161.2s. Number counted, 169. Lapses: omission of a *line*.

C. *Attention on Writing.*

One trial.

(*a*) Counting lower than before and more rapid. Both counting and writing more automatic than before. Ease in experiment with but little sense of certainty as to introspections.

(*b*) A uncertain as to nature of cue for writing. Probably hand-motor control with subsidence of lip-motor and visual control.

(*c*) Time required for writing, 59.6s. Number counted, 116. Lapses: omission of word ('it'), 1; frequent telescoping of letters.

Summary.—The writing-cue was auditory-vocal-motor with control visually by the seen result of writing. When attention went to writing, however, there was subsidence of awareness of such control and the process became more automatic. Attention went more naturally to writing than to counting, the latter being a highly automatic process. Attention was enforced on counting by attending to lip and throat movements; on writing, by no particular method, other than ignoring the counting process. Time of writing was greatly increased when attention was on counting or undirected; it approached the normal when attention was directed to writing. When attention was on counting, one whole line of verse was omitted without A being aware of the lapse.

II. Reading Mentally and Writing.

(Normal time for day, November 14, 56.2s.)

A. *Attention Undirected.*

One trial.

(*a*) Experiment required great effort. Natural disposition was to give attention to writing. Attention, however, oscillated from verse to list of words read with marked tendency for the eye to move to the right of the column of words read, that is, toward the writing.

(*b*) Writing-cue was visual imagery of words, although image and writing overlapped somewhat. There was a successive series of visual percepts (words read) and visual images (writing-cues). Three of the lapses that occurred were spoken of as reported in visual and kinæsthetic terms. Only one of them was reported in detail.

(*c*) Time required for writing, 61.4s. Number of words read, 20. Lapses: 6; omission of letter-part ('m,' 'n,' 'u'), 4; omission of letter ('e' and 'i'), 2; frequent telescoping of letters.

B. *Attention on Reading.*

One trial.

(*a*) Attention oscillated but was on the reading at beginning of the test. Consciousness was largely perceptual. Associative processes set up by list.

(*b*) Writing-cue. A pushed through writing with a brief series of visual images. There was a greater tendency than in II A to use auditory-verbal-motor images, this being particularly the case when A was in difficulty. A spoke of many lapses, which he was unable to specify, as reported kinæsthetically. (See test C below.)

(*c*) Time required for writing, 149s. Number of words read, 82. Lapses: 3; omission of letter, 2; repetition of letter-part, 1; telescoping of letters.

C. *Attention on Writing.*

One trial.

(*a*) Attention oscillated. The reading was done in the gaps between the lines of writing. Attention was enforced on writing by auditory-verbal imagery.

(*b*) Writing-cue. A noticed a distinct shift in control from visual to auditory-verbal-motor imagery. A little visual imagery persisted but it followed rather than preceded writing.

(*c*) Time required for writing, 79s. Number of words read, 25. Lapses: 1; omission of letter, 1.

Summary.—Although the distracting process in II was visual, there emerged, in contrast to I, anticipatory visual cues, due probably to the loss of the perceptual visual control of I. The possibility of very rapid oscillation of attention from visual stimulus to visual image was shown by the low time-reading in A, where this method was employed. When, however, attention went to counting, the oscillation of attention was retarded and the time-reading greatly increased. Attention was enforced on writing by using an auditory-vocal-motor control.

III. Reading Aloud and Writing.

(Normal time of writing for day, July 10, 52.6s.)

A. *Attention Undirected.*

One trial.

(*a*) There was marked oscillation of attention.

(*b*) The writing-cue was auditory-vocal-motor with some anticipatory visual imagery but very little visual report. On the whole, the report from writing was not as distinct as usual. A few words were written automatically by the hand.

(*c*) Time required for writing, 93.6s. Number of words read, 60. Lapses: 2; omission of letter-part, 2.

B. *Attention on Reading.*

One trial.

(*a*) Attention held to reading by effort. The problem was to keep it off the writing. There was a strong tendency to speak the words being written.

(*b*) The writing-cue was auditory-vocal-motor, and the words came in phrases, a cue once given was sufficient. There was a dim visual report of words in groups. The kinæsthetic report from the hand was not very distinct. In normal writing this report is not particularly distinct, nor are such sensations significant.

(*c*) Time required for writing, 144.6s. Number of words read, 164. Lapses: 4; omission of letter-part, 2; repetition of word, 1; repetition of letter, 1.

C. *Attention on Writing.*

Two trials.

(*a*) First trial. A read in a very low hesitating voice. Second trial, oscillation of attention, A read in a low hesitating voice. Less confidence than in first trial.

(*b*) First trial. The writing-cue was anticipatory visual cue with a less noticeable auditory-vocal-motor accompaniment. There was lack of awareness of movement. Second trial. There was visual anticipation as before, but less confidence. A suspected that he had omitted one line (a false suspicion). There was a feeling of uncertainty as to longer words.

(*c*) Time required for writing: first trial, no report; second trial, 77.2s. Number of words read, no report. Lapses: first trial, 6; omission of letter-parts, 3; omission of word, 1; repetition of letter, 1; exchange of letters, 1. A reported that in several places he was not sure of having made the required number of strokes. Lapses: second trial, 2; omission of letter-part, 1; A originally wrote, 'twenty-one' instead of 'twenty-eight' and omitted the last two words of line; then returned and crossed out 'one' and wrote in the final words of line. 'One' was a repetition of word, two lines above.

Summary.—Some visual material was evident in every test, particularly when attention was on writing, on which occasion the writing-cue was visual anticipation. On the whole, the

results in III are very similar to those of II; the addition of overt articulation of words read changed results but slightly. There was a strong tendency to speak aloud the words that were being written. As before, there is great increase in the time-reading when attention is on reading; but little increase when attention is on writing.

IV. COUNTING ALOUD AND WRITING. BLINDFOLDED.

(Normal time for day, November 21, 57s.)

A. *Attention Undirected.*

One trial.

(*a*) Marked oscillation of attention. But counting more nearly automatic than writing.

(*b*) Writing-cue was visual and anticipatory. When eyes are open hand takes the initiative and the eyes are used in correction; when the eyes are closed the functions are exchanged.

(*c*) Time required for writing, 72.6s. Number counted, 94. Lapses: 1; repetition of letter-part.

B. *Attention on Counting.*

One trial.

(*a*) A more difficult experiment than A for counting, particularly in the case of the lower digits, is automatic. The experiment as a whole less difficult than when eyes are used. Counting was enforced by listening to sound of voice.

(*b*) The writing-cue was visual and anticipatory, one or more words usually seen in 'own' handwriting at the beginning of line on a poorly illumined surface. Some articulatory imagery was also used at beginning of lines.

(*c*) Time required for writing, 87.4s. Number counted, 149. Lapses: 4; omission of letter or letter-part, 3; repetition of letter-part, 1.

C. *Attention on Writing.*

Test not tried.

Summary.—The results from IV are comparable with those obtained in I. As these tests were tried the same day, IV had the benefit of practice which may account for the lower time-readings. There is much visual material, as was true also in II and III, and also, probably, in I. As in every other test, attention to the distracting process raised the time-reading.

V. LISTENING TO THE READING OF A LIST OF RHYMING WORDS AND COUNTING MENTALLY THE NUMBER OF TIMES ONE PARTICULAR WORD OCCURS WHILE WRITING.

(Normal time for day, July 11, 52.6s.)

A. *No Directions as to Attention. Eyes Open.*

One trial.

(*a*) Distraction was perfect. But in order to write, attention had to go to hand. There was oscillation of auditory perception and auditory image.

(*b*) Writing-cue was auditory. Perceptual visual control felt to be slight. No hand-automatism.

(*c*) Time required for writing, 90s. Counted 32 out of 34 times word was read. Lapses: 0.

General Summary.—Attention went normally to writing rather than to distraction processes. Both reading and counting proved to be more automatic than writing. Counting was particularly automatic. Attention was focused on counting by employing anticipatory auditory-vocal-motor images or by concentration on auditory sensations. Attention was enforced on writing by vocalization and, at times, by visual report or anticipation. There was a stronger tendency to articulate aloud words being written than to write words read (so also in the case of D).

There are comparatively few lapses, particularly those of persistence. (A's record should be compared with D's.)

Distraction Experiments. Verse.

SUBJECT H.

(Average time of writing verse, after it had been perfectly memorized (14 trials), 80.23s.; M.V., 3.44s.)

I. Counting Aloud and Writing. Eyes Open.

(Normal time of writing for day, December 15, 81.4s. and 78.6s. (two trials); normal rapidity of counting, December 15, 173 in 60s. and 45 in 9s. (one trial).

A. *Attention Undirected.*

One trial.

(*a*) Counting judged to be more automatic than writing. In changing 10's there was hesitation, particularly from 100 on. After 100 counting was a slower process.

(*b*) There was a lip-motor writing-cue at the beginning of each line. At these points there was hesitation in the counting.

(*c*) Time required for writing, 67s. Number counted, 120. Lapses: 3; repetition of letter-part, 1; and omission of letter-part, 2.

B. *Attention on Counting.*

One trial.

(*a*) Attention reverted to writing only at long intervals. Attention was enforced on counting by bringing to consciousness the articulatory sensations accompanying the counting. H kept her eyes off the writing.

(*b*) There were articulatory writing-cues at infrequent intervals; otherwise the writing was automatic.

(*c*) Time required for writing, 85s. Number counted, 130. Lapses: 12; omission of letter-part, 5; omission of words, 7 (an omission of one whole line).

C. *Attention on Writing.*

One trial.

(*a*) The experiment was a difficult one since the intrusion of the auditory or lip-motor sensations incident to counting distracted attention from the writing. When attention was on the writing the counting became slower. This fact, reported in auditory terms, would again distract H's attention from the writing.

(*b*) An articulatory image was used at times to enforce attention on the writing. Attention was also enforced by concentration on the visual perceptual series.

(*c*) Time required for writing, 73.4s. Number counted, 116. Lapses: 2; omission of letter-part, 2.

Summary.—Both writing and counting seemed well automatized. They could be carried on simultaneously without difficulty, even, perhaps, with some facilitation of the writing, as shown by the low time-reading in A. The attempt, however, to throw attention on either process caused disturbance and tended to break up the coördination.

II. Reading Mentally and Writing.

(Normal time of writing for day, January 4, 79s.)

A. *Attention Undirected.*

One trial.

(*a*) Oscillation of attention only when writing the first words of each line of verse. Otherwise attention was on the reading. The words of the reading-list were 'lip-motored' and there was a strong tendency to write them. This was evident in the results. The experiment required little effort.

(*b*) An articulatory writing-cue for the first word of every line except the second, for which no cue was needed. Otherwise, the writing was automatic.

(*c*) Time required for writing, 83s. Number of words read, 60. Lapses: 10; omission of letter-part or of letter, 4; repetition of letter-part, 3; transposition of letter, 1; intrusion of words from reading list, 2.

B. *Attention on Reading.*

One trial.

(*a*) A good deal of oscillation of attention. In enforcing attention on the reading there was a perceptible movement of the lips. H spoke the words read with emphasis.

(*b*) No writing-cue was reported, although there was consciousness of the hand. At the beginning of lines there was hesitation. H was conscious of some

of the errors in writing but uncertain whether the report was in articulatory or grapho-motor terms.

(*c*) Time required for writing, 105s. Number of words read, 49. Lapses: 22; omission of letter-part, 4; omission of word, 6; repetition of word, 4 (one word recurs three times); repetition of letter-part, 2; anticipation and repetition of letter, 5; intrusion of letter from list read, 1.

C. *Attention on Writing.*

One trial.

(*a*) Reading was done visually with mental articulation for only a few words. The articulatory process was used, however, to enforce attention on writing.

(*b*) The writing-cue was articulatory.

(*c*) Time required for writing, 92.4s. Number of words read, 18. Lapses: 7; omission of letter-part, 3; repetition of letter-part, 4.

Summary.—Comparison of the time readings in II and I shows that visual distraction caused greater disturbance than did articulatory distraction. This disturbance may be due to the loss of visual perceptual guidance present in I but not in II, or it may result from the fact that the visual distracting process was a less automatic one than the articulatory. As in I, concentration of attention on either process introduced additional disturbance. There is a great increase in the number of lapses compared with I. Incipient articulation was employed as a method of enforcing attention.

III. Reading Aloud and Writing.

(Normal time of writing for day, January 11, 84s. and 85s.)

A. *Attention Undirected.*

One trial.

(*a*) This experiment was found to be more difficult than II. H broke down at the end of the fourth line of writing and was unable to find writing-cue, although reading was continued for two minutes after break. H began reading in a loud voice which was gradually lowered.

(*b*) Writing-cue was articulatory at intervals.

(*c*) Time required for writing, not reported. Number of words read, 46 before break. Lapses: 8; repetition of word, 1; repetition of letter-part, 2; omission of letters, 2; intrusion of letter from reading list, 2; one word an undecipherable jumble of letters.

B. *Attention on Reading.*

One trial.

(*a*) H was conscious of writing only at long intervals, perhaps only when writing capitals. She was absolutely unconscious of the errors made in the writing, although aware, when writing stopped, of the fact.

(*b*) The writing was largely automatic. An articulatory cue was used in writing the first word of the third line but no other cues were recalled.

(*c*) Time required for writing, 69s (two lines of verse are omitted). Number of words read, 71. Lapses: very difficult to classifiy; the fourth and fifth lines of the verse are altogether wanting; an extra line inserted is a jumble of word-parts; besides these errors there is repetition of words, 2; repetition of syllable, 1; repetition of letter-part, 2; omission of word, 1; omission of letter-part, 3; anticipation of word, 1; anticipation of letter-part, 1; other lapses indeterminable, 3.

C. *Attention on Writing.*

One trial.

(*a*) This experiment was still more difficult than the preceding. The reading was not at all automatic and was done in the intervals between writing. H read very slowly. There was marked oscillation of attention. To enforce attention on writing H was obliged to articulate incipiently, occasionally even overtly, every word written.

(*b*) The writing-cue was articulatory.

(*c*) Time required for writing, 98.2s. Number of words read, 23. Lapses: 2; omission of letter-part, 2.

Summary.—The results from III give evidence of considerable writing automatism, neither writing-cue nor writing report being present except at rare intervals when attention was off writing. Since, however, the distracting process was not at all automatic, when attention was directed to writing, marked oscillation of attention occurred. In the latter case attention was enforced on writing by incipient, sometimes overt, articulation.

IV. Counting Aloud and Writing. Eyes Blindfolded.

(Normal time of writing for day, January 18, 83s. and 80s.; normal rapidity of counting, January 18, 136 in 60s.)

A. *Attention Undirected.*

(Results and introspective report show that attention was actually on the counting. The report is consequently given under B.)

B. *Attention on Counting.*

One trial.

(*a*) No effort was required. Attention was largely on counting. There was very little consciousness of writing, not even a kinæsthetic report from the moving hand.

(*b*) At long intervals there was an articulatory writing-cue.

(*c*) Time required for writing, 82s. Number counted, 140. Lapses: 7; repetition of letter-part, 4; omission of letter-part, 2; omission of word, 1.

C. *Attention on Writing.*

One trial.

(*a*) The experiment was difficult. H counted ten before beginning to write and after beginning counted very slowly.

(*b*) For the first line H used an articulatory cue which conflicted with the counting articulation. In the second, third, fourth and fifth line there was a visual cue preceding writing; short words were usually seen as wholes. In the sixth line there was a return to the articulatory cue.

(*c*) Time required for writing, 121s. Number counted, 45. Lapses: 7; omission of letter, 3; omission of letter-part, 2; repetition of letter, 2.

Summary.—In IV C, in contrast to I C and II C, visual ideational cues appeared in the effort to force attention on writing. The result is comparable to that given under I C, where attention took visual perceptual form. The use of an ideational visual series demands, however, greater effort than was noticed in I C. In IV B the loss of the visual perception of the result, facilitated concentration on the counting. This again parallels the results in I B where H voluntarily kept her eyes off the writing.

V. This test was not tried on H.

General Summary.—Attention went naturally to the distracting process, for writing was the more automatic act. When directed to writing, attention was enforced visually unless the distraction was a visual one; under the latter conditions strong articulation was used to force attention on writing. When distraction was both visual and articulatory, the effort to throw attention on writing caused much oscillation of attention and little success in dealing with the distraction process. Visual solicitation was much more effective than was articulatory, so much so that when H was focusing attention on counting, it was necessary to ignore the visual perception of the writing. Concentration on writing by use of visual anticipation was a highly effective method of enforcing attention but required great effort. A visual-articulatory distraction operated in reducing writing to automatism; many lapses occurred and the process finally broke down altogether.

In general, the automatic character of the writing act was evidenced by the number and character of the lapses. There were many errors of repetition and of omission. In the case

of the latter many were due to similarity in graphic movement so that H probably had the impression of having written the letter or letter-part. Where words were omitted there were often substitutionary repetitions. Exchange of letter-parts occurred occasionally—a purely motor lapse. The solicitation of attention by visual distraction was evidenced by the tendency in reading to insert words or letters from the list read.

3. Tables IV. and V. Tabulating Results from Seven Subjects.

Table IV.

Comparative Table. Distraction Tests. Writing of Verse.

(Key, see note 13.)

No. of Experiment.	Control. Subject B.	Time of Writing in S.	No. of Lapses.	No. Read or Counted.	Control. Subject R.	Time of Writing in S.	No. of Lapses.	No. Read or Counted.
I.[9] A.	Avm, tactile and Grm-s-r	130.8	3	120	Avm	73.5	3	133
I. B.	Avm-i-a	138.4	0	130	Avm, at intervals	72.6	3	148
I. C.	A-i-a	117.5	3	107	Avm, word by word	78.5	2	109
II.[10] A.	Avm, auditory distinct	61.2	4	39	Avm, at intervals	80	3	
II. B.	A, at long intervals	61.7	3	86	Grm-s-r, at intervals	68	1	
II. C.	Avm (double) and A-s-r	64.3	1	26	Avm, by letters & syllables.	66	3	
III.[11] A.	Avm, Grm-s-r, A-s-r	104	10	32	Avm	84	3	
III. B.	Avm	146	16	82	Avm	80.4	4	
III. C.	Avm, A distinct	75	8	26	Avm, word by word	85.4	6	
IV.[12] A.	Avm, at intervals	94	10	72	Avm, V-i-a, slight	78	2	139
IV. B.	Avm	109	16	123	Avm, at intervals	64.4	6	151
IV. C.	Avm, word by word	112	12	87	Grm-s-r, at intervals	82.2	3	150
	Subject H.							
I.[9] A.	Vm, at intervals	67	3	120				
I. B.	Vm, at intervals Ignored V-s-r	85	12	130				
I. C.	Vm, V-s-r	73.4	2	116				
II.[10] A.	Vm, at intervals	83	10	60				
II. B.	Automatic	105	22	49				
II. C.	Vm	92.4	7	64				
III.[11] A.	Vm, at intervals H. broke at end of 4th line		8					
III. B.	Automatic One line omitted	69+	15	71				
III. C.	Vm, word by word	98.2	2	23				
IV.[12] A.	See B. below							
IV. B.	Automatic	82	7	140				
IV. C.	Vm, V-i-a	121	7	45				

[9] Counting aloud.

[10] Reading mentally.

[11] Reading aloud.

[12] Counting aloud blindfolded.

[13] A, attention undirected; B, attention on distracting process; C, attention on writing. Avm, auditory-vocal-motor; Vm, vocal-motor; A, auditory; V, visual; Grm, grapho-motor; s, sensational; i, ideational; a, anticipatory; r, report.

TABLE V.

COMPARATIVE TABLE. DISTRACTION TESTS. WRITING OF VERSE.

(Key, see note 18.)

No. of Experiment.	Control. Subject D.	Rapidity in S.	No. of Lapses.	No. Counted or Read.	Control. Subject A.	Rapidity in S.	No. of Lapses.	No. Counted or Read.
I. A.[14]	V-s-r	73.2	0	123	Avm, V-s-r	113	2	169
I. B.	Avm, tactile report	68	1	108	Avm, V-s-r	161.2	6	169
I. C.	Avm, V-s-r	67.6	0	131	Grm-s-r, V-s-r, probably	59.6	2	116
II. A.[15]	Avm, slight V-s-r Lapse of Grm-s-r	66.8	2	39	V-i-a	61.4	6	20
II. B.	Avm	77.4	4	68	V-i-a, Avm	149	4	82
II. C.	Avm, strong V-i-r	83	1	33	Avm, slight V-i-a	79	2	25
III. A.[16]	Avm, V-i-r	74	4	80	Avm, V-i-a	93.6	4	60
III. B.	Avm, at intervals	79.8	5	79	Avm, V-i-r	144.6	4	164
III. C.	Avm, V-i-r	79.2	2	58	V-i-a	77.2	3	
IV. A.[17]	V-i-a, at places	67	3	132	V-i-a	72.6	1	94
IV. B.	Avm, V-i-r	85	2	107	V-i-a, Avm	87.4	4	149
IV. C.	V-i-r, Avm	62.8	3	117				
	Subject Y.				Subject S.			
I. A.[14]	V		3		V-i-a, V-s-r	182	0	209
I. B.	Vm, V-i-a	178	1	160	V-i-a, Grm-s-r	127.8	2	189
I. C.	Vm, V-i-r	130	4	119	V-i-a, V-s-r	112	3	179
II. A.[15]	Vm, V-i-r, V-s, of situation	101.4	4	39	Vm	104.4	1	52
II. B.	Vm, V-i-a & V-i-r, at places	147.6	8	39	Vm	92	3	58
II. C.	Vm, Grm-s-r	103.8	2	10	V-i-a, V-i-r	92.5	1	37
III. A.[16]	Vm	168	14	44	Vm, V-i-a	160	9	95
III. B.	Vm	155	16	70	V-i-a, Vm	127.5	9	73
III. C.	Vm	158	8	70	V-i-a, V-i-r	106	2	49
IV. A.[17]	Vm	163	11	86	V-i-a, Vm	103+	1	146
IV. B.	Vm	178	10	120	Vm, at places	109.7	1	153
IV. C.					V-i-a, Vm	120.4	2	158

[14] Counting aloud.

[15] Reading mentally.

[16] Reading aloud.

[17] Counting aloud, blindfolded.

[18] A, attention undirected; B, attention on distracting process; C, attention on writing. A, auditory; V, visual; Grm, grapho-motor; Avm, auditory-vocal-motor; Vm, vocal-motor; i, ideational; r, report; a, anticipatory; s, sensational.

4. General Conclusion with Discussion and Comparison of Results from Different Subjects.

(Tables IV and V.)

The main difference brought out by a comparison of the records of the different subjects is the varying extent to which writing is automatized. Much grapho-motor automatism was evident in the work of B, R and H; little in that of A, S, D and Y. In the case of the first three, *attention* went normally to the *distracting* process; in the case of the last four, *attention* went normally to *writing*. B and H reported a strong tendency to write the words they were reading; A and D, a tendency to articulate aloud the words they were writing.

(*a*) The whole course of the experiment goes to show that *attention* is concentrated on the *least automatic* process.[19] The focusing of attention on writing, means for B, H and R an intensification of some form of *anticipatory* cue; for D, A, Y and S, usually, concentration on *report* from writing either in its kinæsthetic or visual form. The evidence for such conclusion is thoroughly adequate, although failure to follow instructions in giving attention to the required process sometimes occurred.

A review of the methods employed by each subject in enforcing writing-consciousness will emphasize the conclusion. B resorted usually to some form of auditory or vocal intensification. He either emphasized the auditory aspect of vocal cue or listened to the sound made by the pen; or else doubled or retarded the vocal cue.[20] H concentrated on the visual side of the situation when articulation was used as a distraction, even resorting, when writing blindfolded, to the laborious and, for her, exceedingly difficult method of visual anticipation. When visual distraction was used she made use of more than the usual number of vocal-motor cues. R attempted at different times various methods of enforcing the writing-consciousness; retarding and emphasizing the auditory-vocal-motor

[19] See Angell and Moore: 'Reaction-Time: A Study in Attention and Habit,' *Psychol. Rev.*, III., p. 245.

[20] Then were times, even, when B suspected that the slow auditory-vocal-process was following, not preceding, the rapid hand movement.

process, and resorting to visual anticipation in places. In one case (IV C) R concentrated on the kinæsthetic report, which was found to retard writing, a result which should be compared with the disorganization of writing resulting for B when attention went to kinæsthetic or visual report from writing.

D found that concentration on visual report or on contact sensations were effectual methods of keeping attention on writing. A concentrated on hand-kinæsthetic or visual report. S concentrated on writing by an increase in the amount of visual material, both anticipatory and report. The writing process for S is well organized and at times the attention seems concentrated on the *anticipatory* rather than the *report* side of the situation. Y in attending to writing often gave actual attention to the formation of separate letters. Withdrawal of attention from writing results in motor disorganization, so little is the process automatized. Even to write his own name well, requires careful supervision. Of all the subjects, Y showed least automatizing of writing process; D, after Y, showed least automatization. For both S and A there is need of less supervision.

It follows from the conclusion above that the most effective distractions are those which appeal to the least automatic processes; for B, auditory and visual; for H, visual; for R, visual. More or less complete distraction with these subjects tends to render writing more automatic and more rapid. Of the other subjects, articulatory distraction is more effective for S and Y and, probably, visual for A and D. In the case of the latter a visual distraction is far from being complete. More or less complete distraction for the last four subjects tends to inhibit writing, as Y's records show time and again, and as was evident when D attempted to write while performing mental arithmetic.

It is evident that success in meeting the conditions of the test does not depend, except to a very slight extent, upon versatility in the use of various imagery processes nor upon skill in 'shunting' these processes, although such 'shunting' occurs at times. (D, for instance, in I and IV made less use of vocal-motor process than in II and III; H in I and IV had

recourse to visual imagery, etc.) General habits of attention were much more potent in facilitating or retarding operations. Such general habits as ability to oscillate attention rapidly (R and A) or power to diffuse attention over a double process, with intense concentration (D), or slowness or inertia in oscillation, are potent in determining results. Still more influential, however, is the tendency to focus by way of certain processes, so that success is more often achieved by a synchronization or oscillatory conflict of two similar processes having high attentional value, than by 'shunting' so as to avoid imagery conflict. In this way B drives abreast two auditory or vocal processes and S, A and D two visual processes.

(*b*) *Mental Imagery.*—Auditory-vocal-motor or vocal-motor imagery as the main anticipatory writing-control was evident in the case of every subject, although its value was reduced under certain conditions (see S, I). The auditory aspect of this process was pronounced for B, R, A and D and at times these subjects reported purely auditory imagery. The ability to emphasize and, perhaps, to dissociate the auditory aspect of the vocal process from the articulatory expert gave these subjects, at times, an advantage over H, Y and S, all of whom are deficient in auditory control.

The verbal unit as reported by A, B and R was, clearly, the line or phrase. A reduction of this unit to word or even letter, perhaps in consequence of inability to utilize auditory images, increased Y's difficulties. Probably, however, the reduction of the verbal unit is due to innervation difficulties.

Much visual material was evident in the results as reported by A, D, Y and S, for all of whom there is need of visual control, either sensational or ideational. B, R and H have recourse to visual control only occasionally.

Dependence upon grapho-motor anticipation was very slight. Conscious dependence upon grapho-motor report, either visual or kinæsthetic, was particularly evident in the case of D and Y, less so in the case of A and S. Automatic dependence upon grapho-motor control was very marked for B, R and H.

In general, the experiments of Part II bring out the functional value of the different writing 'controls' instead of test-

ing, as was expected, the possibility of different controls by the elimination of one control and consequent 'shunting' to another. Distraction does not eliminate a particular form of imagery, although it often introduces conflicts with it and thus throws it into the foreground; chiefly, however, it tests the tendency to concentrate attention on a particular point in the situation and so indirectly determines the attentional value of different 'controls.'

(*c*) *Lapses.*—Errors occurring in writing are evidences of the lapses or anticipations of attention. It is instructive that many lapses occur in the writing of B (86);[21] in that of H (95); and in that of Y (80). On the other hand, there are few in the writing of D (27); in that of A (38); in that of R (39); and of S (34). Ranking the subjects by reference to number of lapses occurring, and beginning with the subject who gave the fewest, we get the following grouping: D, S, A, R, B, Y, H. It is obvious that the subjects for whom writing was most automatic were more subject to lapses, particularly lapses of a repetitionary character.

B, who furnished an excellent introspective account of the cause of certain lapses occurring in his own writing, reported that lapses which are repetitionary in nature (persistence lapses in Bawden's phraseology)[22] were due to lapses of kinæsthetic report from the hand with a return of attention to writing and consequent repetition of letters or words of which there had been no report. Bawden's explanation of persistence lapses as due to habit is similar.[23] In the case of what may be termed 'graphic stuttering,' or persistent repetition of letter-part or word-part, the automatic character of the process is still further in evidence. The writer is inclined to cite such cases of repetition as due to a self-suggested movement comparable to cases of suggestion where the movement of hand is externally impressed and automatically continued. Evidence of such continued repetition occurs in the writing of H, Y and B.

[21] Only the lapses that occurred in writing the verse are included in this numbering. Such an estimation is made in order to make results comparable. Cases where records are slightly incomplete (A, Y and H) would probably yield a slightly greater number of lapses if the tests omitted should be supplied.

[22] Bawden, 'A Study of Lapses.'

[23] *Op. cit.*, p. 89 f.

The fact that 55 per cent. of the whole number of errors made by B were repetitionary in character taken in connection with the fact that only 21 per cent. of A's lapses were of such character furnishes interesting confirmation of the conclusion that such lapses represent errors arising from the lapse of motor consciousness and originate from breaks in the functioning of an automatic process.

It should be noticed, however, that errors of omission (ellipsis, in Bawden's terminology) are ambiguous, since an omission may be due either (1) to a sense of having written a letter or letter-part on account of its graphic similarity to a movement just made, or (2) to inaccuracy of grapho-motor control, or (3) to attentional anticipation.

Lapses of omission or of repetition constitute the majority of the errors recorded. There occur, however, some intrusions from reading and, occasionally, an exchange of letter-parts, a splitting of a letter so that while eventually it is completely made, it is not made in sequence. The latter lapse is evidently purely motor in origin.

(*d*) The study of the lapses occurring in the writing tests leads naturally to a brief discussion of the meaning of automatism so far as brought out in the experiments under consideration. It is evident that in proportion as distraction tended to be complete, writing control and movement became marginal (less and less distinct) with approach to *automatic writing* as a limit. Such automatism might extend over a letter, part or whole, a word, a phrase or even several lines. The analysis of the data so obtained is of particular interest in the light of recent discussions anent the status of the so-called 'subconscious.'

The bearing upon the point in controversy, namely the merely physiological or psycho-physiological character of automatic writing, is shown by the fact that it occurs most frequently in the subjects who are shown by other tests to be particularly grapho-motor in type. In their case motor organization of the writing habit has been successful to a high degree. Accepting a physiological explanation of automatic-writing, the correlation just mentioned is natural enough; but under the supposition of a detached 'co-consciousness,' it is not clear why

a grapho-motor control should eventuate in automatic writing more frequently than does a visual control, for instance. Nor, again, why automatism should be achieved most frequently in the case of the most habitually written phrases, such for instance as the name of the writer.

It is, of course, quite possible for the advocates of the detached consciousness hypothesis to urge that the experiments just discussed are beside the point, since the constant writing of the test-verse favored the production of a writing-habit, which could indeed be explained in physiological terms; but that the interpretation of those cases of automatic writing, which issue in such novel and meaningful combinations of words as is held to be explicable only by the presence of a selective consciousness, remains unaltered. The writer readily concedes the limited application of the experiments here recorded. Yet the transition from marginal awareness to complete unawareness of writing; the long lapses of writing-consciousness that occurred rarely; the dropping out at various times of anticipatory, report, or memory processes; the occasional loss of a sense of agency bear such close analogy to descriptive accounts of what takes place in automatic writing so-called, that it is difficult to believe that they are products of wholly different conditions. To the writer, a physiological interpretation of the present test, at least, is the only one that has meaning.

To repeat, it is instructive to notice the four forms that lapse of writing consciousness might assume, namely, lapse of memory, lapse of report, lapse of anticipation, or lapse of anticipation and report.[24]

First of all, the memory lapse. In tests that were emotionally disturbing, the reagent often confessed his inability to recall the nature of the writing control, although confident of some sort of anticipatory or 'report' cue. The evanescent character of many images when recalled retrospectively was often commented upon. The fleeting character of the image was described metaphorically by speaking of it as a 'flash' or a 'flicker' or comparing it to a 'fish-like darting in and out

[24] Cf. Jastrow: *The Subconscious*, p. 450 f.

of the stream of consciousness.'[25] At other times the subject simply said 'I've forgotten.'

The possibility of such memory lapses, throws, of course, suspicion upon those instances in which the reagent rather confidently reported that he had no knowledge of what he had written, this suspicion being particularly cogent in the case of the longer tests where the introspective account was delayed somewhat. That all failures to report awareness of the writing were not due to memory lapses, may, however, be concluded from the following facts, cited already in another connection: (1) Errors occur such as are easily explicable as due to lapse of awareness, for instance the repetition of words or letters through lapsing of the kinæsthetic or other report of the movement. (2) There is a change in the appearance and size of the writing as it approaches automatism. (3) The lapse of awareness of some particular 'control' process, usually present, gives a peculiar quality to consciousness, the cause of which may be determined by a process of elimination.

Granting that not all lapses of consciousness are due to lapses of memory, one still needs to distinguish lapse of anticipation of movement from lapse of report of movement made. In general, the course of the experiments has tended to establish the very great value of 'report' processes, both kinæsthetic and visual, in control of movement, so that in a way the distinction is a false one for all 'report' processes may in fact be also 'anticipatory' so far as they constitute the cue for the ensuing movement. In such a case, then, the distinction has only descriptive value. But the distinction has, also, functional value; for writing remains voluntary even though there be more or less of a lapse of report; on the other hand, in proportion as the words written are reported but not anticipated, writing remains conscious but becomes non-voluntary.

In ordinary writing there is usually both anticipation, chiefly of a vocal-motor type, and also a report; but with growing automatism of movement, 'report' processes become marginal except in case of conflict. Probably a lapse of 'report,' to a

[25] And the 'psychological angling' for such 'cues' demands much practice and alertness.

greater or less extent, is most frequent in writing on topics that demand concentration on the meaning to be conveyed, for in such cases interest is on the anticipatory side and not at all on the expressive.[26] Such a lapse of report from consciousness may be carried to a great extent without impairing the sense of agency. If, however, the topic written upon encourages the use of habitual sequences of words, there may be frequent lapse of anticipatory cues, except those of kinæsthetic or visual report. Such a reduction of anticipatory content would probably occur somewhat frequently in the case of one for whom mental articulation is well automatized, and would be accompanied by loss of the feeling of agency, though the writer would remain conscious of his movements. Such cases of automatic writing (cited frequently in the literature of the subject) represent approach to automatism from the anticipatory side; such automatism could probably be induced in D and A among the subjects of the present test. Reagents such as B and R, with whom there is normally less consciousness of movement, with lapse of anticipation would lose all consciousness of writing and furnish examples of the true automatic writing, so-called in the literature of the subject.

Habit, automatism, the physiological, represents the final limit of the psychical. Completely achieved automatism is physiological and motor; but imperfectly achieved automatism retains the character of all consciousness and is sensory in character. The approximation to automatism (minimum of awareness, waning of attention) may occur on the visual or auditory side as well as on the kinæsthetic. For the writer, the visual-auditory flash of the initial letter of her name that came, right or wrong, in response to the signal in the tests of Part III, was as automatic as the jerk of H's hand in penning the first stroke of her name. In both cases conflict or retardation of response brought the ' cue ' or ' report ' to the focus of consciousness.

A comparison of the rapidity with which writing went on

[26] Compare, for instance, your penmanship in a formal application for some social favor, with the dishabille in which it appears in your jotting down of your latest inspiration! The chirographic result of much preoccupation with weighty matters gives color perhaps to the charitable maxim that makes poor penmanship a characteristic of ' genius.'

under distraction, if the records are taken as a whole and test V omitted, results in the following ranking: D, R, A, B, H, S, Y. Rapid writing in such experiments was mediated more successfully by automatism on the side of the distracting process than by partial writing automatism; for in the latter case the alternate lapse and return of attention to writing caused disturbances. Complete surrender to the writing automatism gave, however, low time-readings for B (in II and V), R and H. This surrender occurred more frequently in the case of R than in the case of B. For the latter, supervision is withdrawn from writing only when distraction is complete; but when this happens B's rapidity increases greatly. The fact that both counting and reading were automatic processes for D explains the rapidity with which she wrote under distraction. The intense concentration demanded and the excitement of the experiment served several times (see record for name) to induce a writing rapidity greater than could be achieved under normal conditions. The high value of counting and reading as distracting processes, articulation being particularly distracting, explains the difficulties of Y and S in meeting the test and their slow progress in writing. Unpleasant emotional disturbances were, however, much more noticeable in the case of Y and he reported actual conflict of hand and vocal innervation. A test in which Y wrote a series of autographs while doing work in mental arithmetic, the results of which he announced orally, showed that in no case did writing go on while Y was articulating. The results of the mental operations were announced during pauses in the writing, although Y reported that, visually, he was aware of the result even while unable to articulate it.

To a certain extent the table shows that an increase in time of writing is found when attention goes to writing in the case of H, B and R; with a decrease for A and D. This is what one would expect from the general course of the experiments.

5. Distraction Experiments. Writing of Name.

In addition to the experiments in which the subject wrote the test-verse under distraction, a second series was tried under

exactly similar conditions, in which the writing of the subject's name and the phrase, 'University of Chicago' was substituted for writing the verse. As the writing of the phrase gave results that differed only incidentally from those obtained when the name was written, no report will be made upon it; but a detailed analysis will be made of the test on writing the name.

In planning the experiment, it was thought that the writing of the name, being so habitual an act, would give results somewhat different from those obtained when the verse was used in the test. To a certain extent this was found to be true in that the name was more frequently written automatically than was the verse. Certain observers, however, for whom the verse-test had shown writing to be a highly conscious act, rarely wrote even their own name without conscious supervision. On the whole, the results obtained from the test in writing the name are closely comparable with those obtained when the verse was written. The results for this series are tabulated[27] for each subject, and each table is accompanied by a general descriptive summary.

The results from the test on the verse made it evident that certain characteristic changes in the amplitude of the writing movement resulted when writing was modified through the presence of a distracting process or because of changed conditions of attention. In order to get a quantitative statement of this change in the case of every subject, measurements were made of the height of chosen letters[28] in the written name and also of the breadth of all letters taken together, the latter measurement being obtained by getting the horizontal extension of the name as a whole. In order to have some statement of the normal variation in the size of the characters that might be expected during any hour, one experimental session was given to obtaining a series of fifty autographs written under normal conditions. To avoid fatigue, between the writing of every

[27] The abbreviations used in these tables are the same as those employed in Tables IV. and V., viz., A, auditory; V, visual; Avm, auditory-vocal-motor; Vm, vocal-motor; Grm, grapho-motor; i, ideational; s, sensational; a, anticipatory; r, report.

[28] These measurements were taken along the slant of the letter. Characteristic letters were selected for measurement such as capitals and long letters like 'y.'

ten autographs a rest-interval of five minutes was given. From this series the M.V. is calculated. The amount of departure from the normal mean variation in respect either to speed or amplitude of writing may be easily seen by comparing the normal given for any test with the result obtained when there was distraction.[29] For convenience of comparison these normal averages for every subject are listed just above the tabulated statement of the results obtained in the series in which the name was written under the test conditions.

A further test, not tried with the verse, was carried on in connection with writing the name. In this test the experimenter, at the moment that he gave the reagent the signal to begin writing, gave also a number in the hundreds to which the subject added successively 9, 8, 7, 6, 5 and announced the results of the successive additions aloud, all the while writing his name continuously, one autograph below the other. Two series were tried under these conditions; in the first, the subject had his eyes open; in the second, he was blindfolded. The results from these experiments are not included in the tables but are given in the summarizing statement at the close of each table.

Descriptive Summary (*B*).—Unless directed to attend to writing, B usually wrote his name more or less automatically. When attention went to the writing of the name, in contrast to what happened in writing the verse, little use was made of auditory-vocal-motor enforcement. Instead, attention went to the actual movements of the hand or to the visual report, with a consequent break-up of the coördination. The most successful method of enforcement was to concentrate on the *sound* of the moving pen.

The rapidity with which the name was written approximated or was above the normal rapidity unless attention was forced to the grapho-motor or visual report from writing, a form of attention which retarded writing. Many lapses occur in writing the name; the series shows twelve (12).

[29] The measurements given in the table represent one trial each. When dates are given in the notes, the test was *not* completely carried out on one day and in such a case more than one normal is recorded. In every instance the variation from the normal taken the same day is the important thing to observe.

TABLE VI.

WRITING OF NAME—NORMAL CONDITIONS. SUBJECT B.

Average 50 trials, one sitting.

Rapidity, 4.694s.; M.V., .24568s. (Marked R. in table below.)

Length in horiontal, while name, 89.32 mm.; M.V., 7.95 mm. (Marked L-h. in table.)

Height of capitals (4), 8.8 mm.; M.V., .717 mm. (Marked H.C. in table.)

Length in vertical, long letters, 11.55 mm.; M.V., .814 mm. (Marked L-v. in table.)

Height small letters, 1.232 mm.; M.V., .156 mm. (Marked H.S. in table.)

Name—Distraction Experiments.

	R.	Lapses.	No. Counted or Read.	All Measurements in mm. L-h.	H.C.	L-v.	H.S.	Control Processes.
Normal	5	0		76	10	13	1.5	
I.[30] A.	5	0	16	70	9.4	11.5	1.6	Grapho-motor control. Ignored visual perception.
I. B.	5.3	3	20	85	7.7	11	2.1	Automatic, but grapho-motor control strong. Vr ignored.
I. C.	5.4	0	20	94	13	11	1.6	Attention on writing breaks up coördi-
I. C.	7	0		92	15	13.5	1.8	nation.
Normal	5.2	0		68	9.8	14	1.4	
II.[31] A.	4.6	1	5	78	11.3	11.2	3.7	Automatic. Slight Grm-r. and Ar from pen at places.
II. B.	5	3	12	86	10.6	9	2.5	Automatic. Blurred report.
II. C.	7.6	0	3	98	10.5	12	3	Attention on Grm-r and Ar from pen.
Normal	5.2			77	13	15	1.7	
III.[32] A.	7.7	1	6	88	13.2	14	4.2	Cue Avm & Grm-r & Vr. Attention on writing at places.
III. B.	6.4	0	11	110	13.4	13	3.2	No report on capitals. Grm-r at close.
III. C.	10.3	2		116	14.3	20	4.3	Forced attention on Vr. Strained.
III. C.	5.2	0		94	13.2	14	3.3	Attention on sound of writing. Ar.
III. C.	7.4	0		104	17.6	15	5	Attention not on name for capitals; toward close, visual report.
Normal	5.2	0		74	13.6	16	1.6	
IV.[33] A.	5	2	16	93	14.3	14	3.3	Lapse of Grm-r at places. Counting and writing, automatic.
IV. B.	4.4	1	20	82	15.2	13.5	4.4	Cues and report evanescent.
IV. C.	5.5	0	15	110	16	18	3.4	Attention on w. strong. V & Grm-r for capitals.

[30] I. counting aloud and writing. A, attention undirected; B, attention on distracting process; C, attention on writing.

[31] II. reading mentally and writing. A, B, C (see note 30).

[32] III. reading aloud and writing. A, B, C (see note 30).

[33] IV. counting aloud and writing, blindfolded. A, B, C (see note 30).

Distraction, in every case but one (I A), caused a great increase in the horizontal amplitude of the writing, and, usually, an increase in the vertical extent also. This increase was evident whether the writing was automatic or attention focused, by whatever method, on the writing. The one-space letters show the greatest increase in height. See Plate III, 1 (*a*), 1 (*b*).

Mental Addition Test.—For B the mental additions were not difficult and toward the close of the second series (eyes blindfolded) the additions required no effort of attention whatever. Consequently distraction was not complete and there was much oscillation of attention.

Auditory-vocal-motor imagery was used for the work in addition with occasional recourse to visual imagery. When the eyes were open, the writing was controlled visually. When B was blindfolded and attention went to writing, it focused on anticipatory grapho-motor images or on the grapho-motor report. The alternate fixation and lapse of attention resulted in the errors of repetition and exchange of letter-strokes which B's previous writing under distraction had shown. Similarity in the letter-parts of the first initials (W and V) often gave an ambiguity in the grapho-motor report which B remarked upon.

The signatures in both the tests, but particularly in the latter (eyes blindfolded), were written in a large free hand with considerable pressure on pen. B reported an increase in automatism toward close of series and the last autograph covered, horizontally, 144 mm.; the one-space letters averaged 5.5 mm.; the capitals, 15.5 mm., a very great magnification of writing. Compare Plate III, 7 (*b*).

Descriptive Summary (*D*).—The writing of the name was much more automatic than the writing of the verse. When conditions required it, attention was focused on writing the name, as on writing the verse, by concentration on the visual report.

With distraction there was usually a quickening of speed, sometimes to a very noticeable extent. In II and III, when a visual ideational series was run tandem with a visual sensational series, there was a decrease in rapidity.

Table VII.

Writing of Name, Normal Conditions. Subject D.

Averages made from 50 trials, one sitting.

Rapidity, 5.566s.; M.V., .16016s. (Marked R. in table.)

Length in horizontal, whole name, 44.37 mm.; M.V., 2.734 mm. (Marked L-h. in table.)

Length of vertical, initial 'J,' 27.47 mm.; M.V., 1.0912 mm. (Marked L-v. in table.)

Height of other capitals (2), 6.29 mm.; M.V., .576 mm. (Marked H.C. in table.)

Height of one space letters (7), 1.039 mm.; M.V., .1636 mm. (Marked H.S. in table.)

Writing of Name—Distraction Experiments.

	R.	No. of Lapses.	No. Counted or Read.	Measurements in mm.				Control Processes.
				L-h.	L-v.	H.C.	H.S.	
Normal	5.4	0		55	25	7	1.11	
I.[34] A.	5	1	23	50	16	4	.95	Initial cue, contact with paper; counting- and writing-rhythm synchronized.
I. A.	4.2	0	20	40	16	5	1.35	Initial cue, contact, followed by A-i-a.
I. B.	4.8	0	15	42	16	7.2	1.12	Initial cue, contact; heavy pressure on pen; hard to focus counting.
I. C.	4.6	0	21	36	13	4.2	.9	Attention focused on visual report.
Normal[38]	5.3			43	18	7.5	1.07	
Normal[39]	6			61	17	7.2	1	
II.[35] A.[38]	4.8	0	7	53	14	5.2	1.14	Initial Vm cue, a mental mumble; lapse of grapho-motor report; light pressure.
II.[38] B.	5	0	8	55	19	4.2	1.38	Initial V-i-a cue. After that, automatic.
II.[39] B.	5	0	10	40	16	5	1.5	No initial cue but signal. Slight consciousness of Gr.-motor report.
II.[39] C.	8.4	0	11	39	14	4.5	1.18	Avm cue with V-i-r, letter by letter. For one capital a fusion of A. and V. imagery.[40]
Normal	5.4			49	20.5	6.5	.83	Avm cue with A-s-r.
III.[36] A.	5	0	12	48	15.5	6	1.44	No initial cue. Hand 'set' before signal. Writing automatic. Light pressure.
III. B.	6	0	12	31	16.5	3.5	1.11	After initial cue, automatic. Heavy pressure.
III. C.	6.8	0	6	40	12	4	1.1	Avm cue with V-i-r. Heavy pressure.
Normal	5.4			69	21	9.5	.93	
IV.[37] A.	3.9	1		38	10.5	4	1.31	Initial cue V-i-a of 'J.'
IV. B.	5	0	9	36.5	13	4.2	1.35	Counting and writing rhythm synchronized.
IV. B.	4.9	0	9	36	14	4	1.12	Initial cue, a visualized 'l.' Heavy pressure.
IV. C.	4.4	0	15	32	12	4.5	1	V-i-r. Head bent low over writing.
IV. C.	4.2	0	17	29	9	3.2	1	V-i-r; pressure heavy; faint Avm.

[34] I. counting aloud and writing, eyes open. A, attention undirected; B, attention on distracting process; C, attention on writing.

[20] II. reading mentally and writing. A, B, C (see note 34).
[21] III. reading aloud and writing. A, B, C (see note 34).
[22] IV. counting aloud and writing, blindfolded. A, B, C (see note 34).
[23] January 17.
[24] January 24.
[25] This initial, an 'E,' 'sounds as it looks.'

In the whole series but two (2) lapses are recorded.

There was a very great decrease in amplitude of the writing movement both horizontal and vertical in I and IV (articulatory distraction), especially when attention was concentrated on writing. In II A and B, there was an increase in the horizontal movement. Under these conditions distraction was more complete and writing more automatic. The notes, here, are instructive, for D spoke of the lapse of consciousness of the formation of letters and reported awareness only of general direction of hand-movement. It was as though the hand were drawing a straight line outward. (Cf. test below where mental arithmetic was used as a distraction.) See Plate III, 2 (*a*), 2 (*b*).

Mental Addition Test.—D found this experiment an exceedingly difficult and fatiguing one. It was highly disagreeable. Mental addition requires great effort of attention on D's part so that such mental work is a very effective method of distraction for her. With the eyes open the writing remained under *visual perceptual* control, was done *consciously*, and resulted in a *minute even* writing, showing heavy *pressure* on the pen. The average time required for writing the name was about two (2) seconds *above the normal*, a great retardation of speed. There was some tendency to visualize numbers, a visualization which the presence of the writing confused.

When D was blindfolded, the withdrawal of the visual perception of the result, and the blurring of the visual ideational report through the tendency to visualize the numbers resulted in the *withdrawal* of *attention* from writing to a greater degree than in any other experiment. At certain places there was complete lapse of consciousness of the writing of certain letters. The writing was somewhat disorganized and considerably *increased* in horizontal *extent*. The one-space letters were also increased in vertical as well as horizontal extent. The autographs were written *more rapidly* than when the eyes were open and there was much *less pressure* on the pen.

Table VIII.

Writing of Name, Normal Conditions. Subject S.

Averages made from 50 trials, one sitting.

Rapidity, 7.9998s.; M.V., .108284s. (Marked R. in table.)

Length in horizontal, whole name, 89.1 mm.; M.V., 3.9 mm. (Marked L-h. in table.)

Height of initial capital, 6.03 mm.; M.V., .7056 mm. (Marked H.C. in table.)

Height of other capitals (2) averaged, 8.62 mm.; M.V., .576 mm. (Marked H.c. in table.)

Height of final 'd,' 11.04 mm.; M.V., 1.31 mm. (Marked H.d. in table.)

Writing of Name—Distraction Experiments.

	R.	No. of Lapses.	No. Counted or Read.	Measurements in mm.				Control Processes.
				L.h.	H.C.	H.c.	Hd.	
Normal	7.6	0		87	10	10.5	13	
I.[41] A.	8.4	0	29	87	9	9.5	13	Initial V-i-a cue.
I. B.	7.4	0	29	95	7	8.5	13	Initial V-i-a cue. At close slight grapho-motor consciousness.
I. C.	10.3	0	28	87	9	8	9	Initial V-i-a cue. Visual attention.
Normal	8	0		92	8	9.5	12	
II.[42] A.	7.4	0	19	87	8	7	9	Initial V-i-a cue; slight Vm; conscious of grapho-motor report.
II. B.	8.6	1	10	92	8	7.5	8	Initial V-i-a cue. Vm, letter by letter. Attention on writing.
II. C.	9	0	7	72.5	5	6.5	9	V-i-a at intervals; Vm, letter by letter. Conscious of grapho-motor report. Hard pressure on pen.
Normal	8.2	0		98	6	9.5	11	
III.[43] A.	10.9	1	14	99[45]	8	8.2	8	Initial V-i-a cue; then conscious grapho-motor control, both a and r.
III. B.	9	1	12	95[45]	7	7.5	7.5	Initial V-i-a; then Grm-i-a.
C.	10.3	1	10	91	6	9	9	V-i-a at intervals; V-i-r, three times, including all that had been written at moment of report.
Normal	7.6	0		92	8	10	11.5	
IV.[44] A.	8.8	0	24	102	10	11	6	V-i-a throughout.
IV. B.	7.2	0	24	101	8	9	9	Initial V-i-a; rest automatic.
IV. C.	8.3	0	24	92	7.5	8	5	V-i-a and V-i-r,[46] this report was for every letter, a report on the formation of letters.

[41] I. counting aloud and writing, eyes open. A, attention undirected; B, attention on distracting process; C, attention on writing.

[42] II. reading mentally and writing. A, B, C (see note 41).

[43] III. reading aloud and writing. A, B, C (see note 41).

[44] IV. counting aloud and writing, blindfolded. A, B, C (see note 41).

[45] Length increased by repetition.

[46] The visual cue was seen above the paper; the visual report followed in the wake of the pen, flowed with the ink.

Descriptive Summary (*S*).—S used a great deal of visual material in writing his name just as he did in writing the verse. This visual material took both anticipatory and 'report' form. In only one or two cases was there an approach to automatism. Attention went naturally to writing. Enforcement of attention to writing was brought about by an increased use of visual material, usually in the form of a visual report.

With distraction there was usually a retardation in the speed of writing. The notes on the three exceptions to this statement show, on these occasions, a tendency toward increased automatism.

A few lapses occurred in this series; three (3) in all.

S's writing under distraction shows a slight but pretty consistent reduction in size of writing characters. One or two exceptions to this statement occur, but in such cases there was usually evidence of slight approximation to automatism. Without exception, attention on the writing caused considerable reduction in writing amplitude.

Mental Addition Test.—S found it impossible to write while carrying on a successive addition of 9, 8, 7, 6, 5. Four trials were made, with the eyes open in every case, but each time the mental addition inhibited the writing movement. A simpler addition consisting of adding by 7's was then substituted for the more difficult one. The visual perception of the writing proved, however, so distracting that S was obliged to look off the paper in order to add the numbers, which were visualized. When attention returned to writing, S looked down at the paper and made use of a visual cue. There was much oscillation of attention, and very little surrender to writing automatism. There was a slight decrease in the size of the writing characters, except for the one-space letters, increased pressure on pen, and very great retardation of writing-speed.

Descriptive Summary (*Y*).—Y found it very difficult to write even his own name under distraction. Visual, graphomotor, and vocal-motor processes were all utilized in the attempt to maintain writing. Writing under distraction became much more vertical than Y's normal writing and much less finished.

TABLE IX.

WRITING OF NAME, NORMAL CONDITIONS. SUBJECT Y.

Averages made from 50 trials, one sitting.

Rapidity, 3.94s.; M.V., .1112s. (Marked R. in table.)

Length in horizontal, whole name, 51.94 mm.; M.V., 2.98 mm. (Marked L-h. in table.)

Height of initial capitals (2), 8.296 mm.; M.V., .617 mm. (Marked H.C. in table.)

Length in vertical, initial 'Y,' 18.47 mm.; M.V., 1.1344 mm. (Marked L-v. in table.)

Height one space letters, 1.729 mm.; M.V., .201 mm. (Marked H.S. in table.)

Writing of Name—Distraction Experiments.

	R.	No. of Lapses.	No. Counted or Read.	Measurements in mm.				Control Processes.
				L-h.	H.C.	L-v	H.S.	
Normal[51]	4	0		78	10.2	20	1.35	
Normal[52]	4	0		58.5	8.7	19.5	2	
I.[47] A.[51]	5.6	1	16	60	7.7	17	1.37	Introspection baffled.
I. B.[52]	9.4	0	11	51	7.5	12.5	2	Attention on finger movement and some V attention. Counted three before beginning to write.
I. C.[52]	6.7	0	12	58	7.7	15	2.2	Anticipatory Grm cue at start.
Normal[53]	3.8	0		68.5	11.5	22	1.27	
Normal[54]	4.2			61	9.5	19	1.35	
II.[48] A.[53]	4.8	2	9	73	9.5	17	1.93	Vm. V-i-r at close. Oscillation of attention, but attention chiefly on writing.
II. B.[53]	6.2	2	2	66	9.5	17	1.64	V-i-a at start. Vm report. Oscillation of attention.
II. C.[54]	6.9	1		55	9.5	20.5	2.2	Attention on finger movement and V-i-r. Vm, letter by letter. Heavy pressure on pen. Oscillation of attention but no conflict of processes.
Normal[55]	4.4	0		60	12.2	21.5	1.42	Vm at intervals.
Normal[56]	4	0		63	10.2	23	1.78	
III.[49] A.[56]	4.6	0	4	75	10.2	19	2	No oscillation of attention. Superimposition[57] of Vm (throat), letter by letter cue for writing, with V-i-r, and Vm (lip) imagery for writing.
III. B.[55]	7.4	2	7	82	11.5	20	2.18	Vm at places. V-i-r at places. Conscious of grapho-motor report.
III. B.[56]	12.8	2[58]	12	78	8.7	18	1.6	V-i-r series as cue. Difficult.
III. C.	No test							
Normal	4			63	10	21	1.42	
IV.[50] A.	6	1	13	60	8.2	1.8	2.71	Vm, letter by letter. V-i-a at places. Pressure heavy. Oscillation of attention.
IV. B.	6.2	2	10	66	12	22	2.57	V-i-a; incipient impulse to pronounce. Pressure heavy. Counted three before starting to write.
IV. C.	No test							

[47] I. counting aloud and writing, eyes open. A, attention undirected; B, attention on distracting process; C, attention on writing.

[48] II. reading mentally and writing. A, B, C (see note 47).

[49] III. reading aloud and writing. A, B, C (see note 47).

[50] IV. counting aloud and writing, blindfolded. A, B, C (see note 47).

[51] January 22.

[52] January 29.

[53] February 6.

[54] February 13.

[55] February 20.

[56] February 27, Y. was tired at time of test.

[57] A novel experience for Y.

[58] At close of name there was graphic stuttering. Throughout, writing is nervous.

The writing-rapidity was greatly retarded by distraction.

Lapses occurred in the writing of nearly every autograph when distraction was present. The series shows, in all, thirteen lapses (13). In order to write well, Y reports that care is needed and a certain amount of visual control. Plate 5 (*a*), 5 (*b*).

With distraction there was a reduction in writing-amplitude (except for the one-space letters) in all cases but III A and B, IV B and, in horizontal extent, II A. The increase in the height of the one-space letters was noticeable throughout.

Mental Addition Test.—Y found the mental calculations involved in the additions easy and not at all fatiguing. Although vocal-motor associations were utilized in adding he showed considerable facility in visualization, a method which was used to hold the sums resulting from the addition. For instance, if he had lost, through distraction of attention, the number that was next to be added in the series, he could recall the visualized results of the additions and so calculate the next number to be added in the series. He was able to review results back four or five steps.

For writing his name Y used mental vocalization, letter by letter. To write at all required conscious supervision and it was not possible for Y to write while announcing aloud the results of his calculation. At times, he reported that he was aware of the result of his addition but could not enunciate it without inhibiting writing. Without giving very careful attention, involving usually numerical calculation, Y found it impossible to write correctly the closing strokes of his name (um).

In one place, eyes open, where Y made an unsuccessful effort to add while writing, the resulting signature shows distinctly the effect of inhibition. The initial capital is reduced from the six or nine millimeters of the normal autograph to barely three millimeters. When the eyes were closed inhibition in extent of movement was even more evident than when eyes were open; writing was more frequently disorganized; and there was heavier pressure upon the pen. Plate III, 7 (*a*).

Descriptive Summary (*R*).—The writing of the name was a highly automatized act for R. To focus attention on it required visual or auditory-vocal-motor enforcement.

With distraction there was usually a quickening of speed. This quickening was least noticeable when attention was on the writing. Only two lapses are recorded in the series of tests on the name.

Throughout the experiments there was a very great increase in the height of the one-space letters. The normal mean variation in the length of the name as a whole is so great that only the tests in IV and in I B and III C show an increase in horizontal amplitude above the normal mean variation. In IV, however, this increase is very great. There is little tendency towards increased height of the capital letters, in fact, in several cases there is a decrease. This is probably due to the fact that R consciously begins her name with a big 'flourishing' capital. Plate III, 3 (*a*), 3 (*b*).

Mental Addition Test.—The test requiring successive mental addition of 9, 8, 7, 6, 5 to the given number, eyes open, R found very difficult. There was no lapse of consciousness of writing name while performing the work, which was done by mental vocal-motor processes. The writing of name was well done, somewhat smaller than usual, and greatly retarded in speed. Simpler work in addition was substituted and the test repeated, another day. On this occasion the writing tended to become automatic with increase in horizontal magnitude (one signature is 165 mm. in length); increase in height of one space letters; and decrease in height of capitals.

Descriptive Summary (*A*).—The name was rarely written automatically. Attention usually went to the writing of it.

Table X.

Writing of Name, Normal Conditions. Subject R.

Averages made from 50 trials, one sitting.

Rapidity, 6.116s.; M.V., 174s. (Marked R. in table.)

Length in horizontal, whole name, 88.42 mm.; M.V., 9.27 mm. (Marked L-h. in table.)

Height of capitals (2), 9.42 mm.; M.V., .9007 mm. (Marked H.C. in table.)

Height one space letters, 1.66 mm.; M.V., .178 mm. (Marked H.S. in table.)

Writing of Name—Distraction Experiments.

	R.	No. of Lapses.	No. Counted or Read.	Measurements in mm. L-h.	H.C.	H.S.	Control Processes.
Normal	6.2			104	15	2.7	
I.[59] A.	5	0	27	119	16	4.5	Grm report. Slight visual control.
I. B.	5.3	0	26	105	16.3	4.9	Grm report. Slight visual control.
I. B.	5.2	0	30	102	14.3	4.4	Grm report. Slight visual control.
I. C.	5.4	0		103	16.3	3.6	Visual attention. Voice lowered.
Normal	No record						
II.[60] A.	7	0		92	9	2.5	Initial Avm; then automatic.
II. B.	6.6	0		92	11	3.1	Nearly automatic.
II. C.	9	0		87	7.3	2.4	Avm, letter by letter with V-i-r.
Normal	6.2	0		104	15	2.7	
III.[61] A.	5.2	0		95	12.2	4.1	Conscious grapho-motor control.
III. B.	7	0		98	10.5	3.5	Avm, at intervals. Not automatic.
III. C.	4.8	1		124	10.6	3.6	Almost automatic. Attention on reading.[63]
III.[64] C.	6	0		112	12	4	Attention on grapho-motor sensations but little meaning to them.
Normal	6.4	0		83	12.3	2.1	
IV.[62] A.	4.8	1	26	113.5	11.3	3.42	Grm control with V-i-a at intervals.
IV. B.	5.2	0	30	154	13.3	3.54	Initial Avm cue.
IV. C.	6.3	0	31	108	11.3	3.64	V-i-a initial cue.

[59] I. counting aloud and writing, eyes open. A, attention undirected; B, attention on distracting process; C, attention on writing.

[60] II. reading mentally and writing. A, B, C (see note 59).

[61] III. reading aloud and writing. A, B, C (see note 59).

[62] IV. counting aloud and writing, blindfolded. A, B, C (see note 59).

[63] Writing was somewhat disorganized. R did not obey directons.

[64] Throughout distraction experiments I and III there was a slight increase in pressure on pen.

The enforcement of attention to writing was generally a visual enforcement.

The speed of writing was usually increased by distraction; but the effort required in throwing attention on the distracting

process always caused a retardation. When attention went to the writing there was little change in writing rapidity except in III C, where there was a shift from the usual visual control to a grapho-motor and contact control which was felt to be strained. Five (5) lapses occurred in the series.

Table XI.

Writing of Name, Normal Conditions. Subject A.

Averages made from 50 trials, one sitting.

Rapidity, 4.178s.; M.V., .19576s. (Marked R. in table.)

Length in horizontal, whole name, 90.76 mm.; M.V., 1.88 mm. (Marked L-h. in table.)

Length in vertical, initial 'J,' 18.36 mm.; M.V., .825 mm. (Marked L-v. in table.)

Height of other capitals (2), 7.01 mm.; M.V., .753 mm. (Marked H.C. in table.)

Writing of Name—Distraction Experiments.

	R.	No. of Lapses.	No. Counted or Read.	Measurements in mm.			Control Processes.
				L-h.	L-v.	H.C.	
Normal	5			105	26	11	
I.[65] A.	4	0	18	76	20	7.5	Grm and V-s-r control. Att. on writing.
I. B.	5.8	0	22	73	16	8.5	Initial cue was Grm-i. Visual correction.
I. C.	5	0	29	70	20	8.5	V-s-r control, V-i-a slight.
Normal	5.2			96	28	12	
II.[66] A.	4.8	0	6	80	15	9	Attention on writing.
II. B.	5.6	1	8	80	16	9.5	Grm-s-r with V-i-a control.
II. C.	5	0	4	87	17	9	V-i-a. Visual control throughout.
Normal	4.4			111	23	12.5	
III.[67] A.	4.6	1	9	115	22	12	V-i-a and V-i-r.
III. B.	9.4	0	18	138	22	12	No initial cue. A-i-a in center.
III. C.	16.6	1		133	25	10.5	Attention on grapho-motor and contact sensations which are unmeaning. Break-up of coördination.
Normal	5			105	26	11	
IV.[68] A.	5.6	1	20	80	19	8.5	Two lapses reported incorrectly.
IV. B.	5.9	1	24	78	15	9	V-i-a control throughout.
IV. C.							

[65] I. counting aloud and writing. A, attention undirected; B, attention on distracting process; C, attention on writing.

[66] II. reading mentally and writing. A, B, C (see note 65).

[67] III. reading aloud and writing. A, B, C (see note 65).

[68] IV. counting aloud and writing, blindfolded. A, B, C (see note 65).

TABLE XII.

WRITING OF NAME, NORMAL CONDITIONS. SUBJECT H.

Averages made from 50 trials, one sitting.

Rapidity, 10.662s.; M.V., .3766s. (Marked R. in table.)

Length in horizontal, whole name, 1033.5 mm.; M.V., 6.8 mm. (Marked L-h. in table.)

Height of capitals (4), 4.13 mm.; M.V., .33 mm. (Marked H.C. in table.)

Length of final 'y's' (2), 4.07 mm.; M.V., .555 mm. (Marked L-y. in table.)

Writing of Name—Distraction Experiments.

	R.	No. of Lapses.	No. Counted or Read.	Measurements in mm.			Control Processes.
				L-h.	H.C.	L-y.	
Normal	13.4			117	5.8	9	
I.[69] A.	9.4	0	37	106	5	6.2	Vm & Grm-r at places; largely automatic.
I. B.	10	2	25	136[73]	6.5	7	Automatic, after initial V-i-a cue.
I. C.	10.4	0	27	89	4.9	7.5	The first initial is Vm; heavy pressure on pen. Visual attention.
Normal	12.4	0		116	5.4	7.7	
II.[70] A.	10	2	18	141	4	6.5	Automatic.
II. B.	11	2	12	138	5	6.2	Nearly automatic. Attention was enforced on reading by actual lip movement.
II. C.	14.5	1	18	148	5.5	10	Vm enforcement, syllable by syllable.
Normal	12	0		109	4.6	4	
III.[71] A.	No test						
III. B.	10	1	18	134	4.6	3.5	Almost automatic.
III. C.	17.8	0	4	129	6.2	5	Vm enforcement, syllable by syllable.
Normal	11.7	1[74]		120	4	4.5	
IV.[72] A.	10	1	40	125	5	3.7	Automatic. H. not aware what she had written.
IV. B.	No test						
IV. C.	27.4	0	18	148	6.2	9.7	Initial V-i-a cue for first part of name; then attention was thrown on graphomotor report. From this point on writing became large, childlike and rounded, unlike usual telescoped writing.

[69] I. counting aloud and writing, eyes open. A, attention undirected; B, attention on distracting process; C, attention on writing.

[70] II. reading mentally and writing. A, B, C (see note 69).

[71] III. reading aloud and writing. A, B, C (see note 69).

[72] IV. counting aloud and writing, blindfolded. A, B, C (see note 69).

[73] The horizontal measurement would be longer, if it were not for the omission of two letters.

[74] Lapses often occur in H.'s normal writing.

A's writing showed usually a reduction in size when distraction was present. III (distraction both visual and articulatory offered an exception to this statement. Plate III, 4 (*a*), 4 (*b*).

Mental Addition Test.—A, who found the mental additions easy to perform, made use of visual and auditory-vocal-motor imagery in effecting them. When the eyes were used the writing was controlled largely by the visual perceptual series. There was little effort reported; light pressure on pen; and, toward the center of the series, a slight increase in size of writing, with growing automatism. The speed of writing was slightly retarded.

When the eyes were blindfolded the loss of the perceived result of writing was very evident. Writing became at places more automatic than it had been in any other test, increased in size, and was retarded in speed. Lapses occurred frequently. When attention went to writing it took visual form and introduced conflict with the visual imagery used in the calculations. A reported at times a curious sense of the absence of the grapho-motor sensations usually present in writing. (Cf. D, p. 111.)

Descriptive Summary (*H*).—Unless the experimental directions required the focusing of attention on writing, the writing of the name was largely automatic. As in the case of writing the verse, H forced attention on writing her name by concentrating on visual anticipation or visual report, when the distraction was articulatory; by using vocal-motor enforcement when there was visual distraction.

Distraction increased the rapidity with which H wrote her name unless attention was thrown on writing, on which occasions writing became slow and laborious. I C (articulatory distraction) offers the only exception to this statement and in this instance H was able to concentrate perceptually on the visual report.

Many lapses occur in writing the name, ten (10) in this series. Such lapses occur frequently when H is writing with no accompanying distraction. The fifty autographs, taken as a normal series, show seventeen (17) such lapses.

With distraction there was always an increase in the horizontal extent of the writing, except in I A and I C (distraction

articulatory), in which attention was probably focused on visual report. (Attention, perhaps, was on contact sensations and finger movement was used.) There was slight variation in the height of letters measured and such variation as there was, tended toward a reduction in size of writing characters, *unless* attention was focused on *writing,* when writing was greatly magnified. The long time required for writing these latter signatures indicates that the writing in these instances was a hesitant not a running hand. In I C, which offers an exception to this statement, a different method was used to enforce attention and the speed of writing exceeded the normal rapidity.

Mental Addition Test.—This experiment was not tried with H.

It is evident from the foregoing account of the writing of the name under distraction, that the results from this test confirm the conclusions reached from a consideration of the results obtained when the verse was written under distraction. Only a brief repetition of such conclusions will be necessary.

As before the subjects fell into two groups. For B, H and R there was a strong tendency to write the name automatically; but for D, A, S and Y this tendency was very slight, although if the distraction were particularly effective, D and A sometimes succeeded in writing a more or less automatic autograph. Both A and D report on occasion a curious sense of the loss of the normal grapho-motor sensations, a peculiar feeling which to be realized needs to be experienced. As in the test on the verse, there is evidence here that hand-innervation in Y's case demands distinct effort, deliberate volition.

As before, the reagent best concentrated on writing by enforcing the least automatic process. Concentration on writing retarded speed for B, R and H but quickened it for D. The general tendency for mere distraction to quicken writing of the name was noticeable in B, R, A and D. This result is due partly to a surrender to automatism but partly also to what may be called 'psychic pace-making.' For D, the quicker operation (counting for instance) sets the pace and writing is speeded in consequence.

Lapses in writing the name occurred frequently for Y, B

and H; more rarely for D, R, S and A. This parallels exactly the results obtained from the verse.

6. Variations in Size of Writing under Distraction.

It remains to discuss the changes in the size of writing characters under distraction—a subject not touched upon in citing the results from writing the test-verse. A brief reference to the conclusions of other investigators relative to this point may be profitable. As regards the relation of the extent of writing movement to the ease or difficulty of the task imposed on the writer, Diehl[75] found that the extent of the writing stroke decreased under certain conditions of difficulty and also under certain emotional conditions that were unfavorable. He reported an increase in extent when the task was lightened.

Binet[76] in an experiment on modified writing, in which there was substituted for each vowel of the word given to be written the vowel that followed it in the alphabet, found a slight increase[77] in the size not only of the substituted vowels, but also of the unchanged consonants. The amount of increase in size seemed dependent upon the intensity of the mental excitation of the reagent, so that the suggestion is made that the amount of increase in writing-amplitude under such conditions might be used to determine the excitability of the writer. Binet insists that only the excitement induced by the graphic work mentioned can be concluded, without further experimentation, to have the effect mentioned.

In an attempt to analyze the phenomenon Binet urges that modified writing ceases to be mechanical and that the conditions of the experiment provoked an insistence upon the letter to be written, an insistence which produces either a visual or a verbal idea or causes fixation of attention upon the graphic movement. In any case the state of consciousness accompanying the writing is more 'consequential' than under normal conditions. The

[75] 'Ueber die Eigenschaften der Schrift bei Gesunden,' *Psychol. Arbeit.*, Vol. III., p. 1.

[76] Binet: 'L'Écriture pendant les états d'excitation,' *L'Année Psychol.*, 1902, Vol. IX., p. 57.

[77] Binet also cites, but without comment, one case in which modified writing resulted in a reduction in the size of the writing.

intellectual work required in suppression and substitution of letters produced a diffused excitation, but not a disorganizing excitation since it assured a better formation of letters.

Lemaitre[78] and Claparéde[79] repeated Binet's experiment. The former, working with a large number of boys, concluded that the test is a test of excitability. Claparéde, who used himself as subject, explained the result, namely the enlargement of the writing characters, as due merely to the transformation of running into hesitant writing through the arrest of movement, the mental hesitation, induced by conditions such as make running writing impossible.

A study of the tables of the present series with reference to the changes in amplitude of writing introduced by the various distractions makes it evident that the relation between the conditions and the resulting variation in the size of the characters is one of considerable complexity. The measurements given in the tables are, it should be recalled, taken roughly and only on selected letters (except in the case of B's and D's autographs where every letter was measured). The indications, however, are unmistakable.

The following conclusions seem justified: There is an increase in the amplitude of movement as complete withdrawal of attention from writing (automatism) is approximated. This increase in size was evident not only in the autographs obtained from B, R and H, with whom automatic writing resulted frequently, but was also evident in the rare cases of automatic autographs reported by D and A.[80] Such a result is in agreement with Diehl's findings.

Increase in size of characters occurred again whenever attention oscillated in such a way as to induce a separate impulse for the writing of each letter. This result is comparable with that obtained in Binet's test in which running writing was interfered with.

[78] Lemaitre: 'Un Test, basé sur le modification de l'écriture,' *Archiv de Psychol.*, III., p. 389.

[79] Claparéde: 'Remarque sur le grossissement provoqué de l'écriture,' *Archiv de Psychol.*, III., p. 390.

[80] This was particularly true in the tests where mental addition was used as a distracting process. These measurements are not given in the tables.

Decrease in the size of writing resulted in such reagents as A, D, Y and H whenever a distraction process accompanied the writing, *unless* the distraction was so complete that writing approached automatism. Such automatic writing of the name was possible for A and D, although a strong distraction was needed to make it evident. When such automatism occurred in the case of these subjects the increase in amplitude of movement over that found in normal writing was evident, although both A's and D's writing showed a marked decrease in the size of characters under the usual conditions of distraction.

This decrease in the size of writing under distraction is evidently an indication of the effort of attention involved in carryihg on voluntarily two processes and it varies from a slight decrease to very great reduction in size.

In general, this curious result is found, that the experiments marked C in the different series show for B, R and H the greatest increase in size; for D, A and S the greatest reduction. The variation from the normal in these opposite directions was often very considerable, yet both groups of subjects had been instructed to give attention to writing so that automatic writing was shut out by the condition of the experiment. The experimental directions resulted for the first group in a break-up of writing coördination, with a separate impulse for each letter, and in oscillation of attention. The second group responded to the conditions with an intensification of the amount of effort.

Decrease in writing amplitude, frequently found in those reagents whose attention went normally to writing, occurred also, although rarely, in the reagents of the other group. For the form which attention upon writing assumed was also influential in determining writing size. Attention upon the visual report or upon contact sensations tends in general, quite apart from distraction of attention, to reduce the size of writing as the writer's experience has shown her many times. On the other hand, if the writer concentrates upon the sensations coming from the hand or arm an increase in amplitude results. The change here is probably due to use of the finger-movement in the first case, and a wrist- or forearm-movement in the other.

Whatever induces such a change in the musculature used will also influence the size of the resulting writing. A complete treatment of variation in size of writing under distraction should include a separate registration of finger- and hand- or wrist-movement such as was achieved by Judd.[81] Even the crude measurements given in the tables indicate that the variation in the height of the one-space letters is not always in the same direction as the variation in their breadth or the height of the longer letters.

The relation of emotional conditions to writing amplitude in the present series is ambiguous. B, R, D and Y showed strong emotional disturbances. R, B and D found the experiments exciting and interesting; Y found them depressing. But no general correlation can be established between the pleasantness and unpleasantness of the experiment and the variation in the amplitude of the writing. The effort of attention involved was a much more important factor. Both Y and D found the tests required effort and for both there is usually a reduction in size. The emotional conditions in the two cases were however very different.

Closely associated with the change in writing amplitude is that of variation in pressure. No effort was made to measure such variation in pressure. It is, however, frequently evident in the result and was sometimes reported by the reagent as a method of enforcing attention. It is very evident in D's writing where it occurs in connection with decrease in extent of movement.[82]

For variations in writing-amplitude, compare Plate III.

7. Conclusion, Part II.

Briefly, then, in recapitulation:—The reagents fell into two groups. The first group (B, R and H) showed a strong ten-

[81] Judd: 'An Experimental Study of Writing Movements,' *Phil. Studien*, XIX., p. 243.

[82] Diehl (*loc. cit.*) found that increased difficulty in the task imposed increased the amount of pressure; and that increased pressure with decreased size of writing and retardation of speed accompanied writing under unfavorable emotional conditions. See also Freeman: 'Preliminary Experiments on Writing Reactions,' *Psychol. Rev.*, Monog. Sup., Vol. VIII., No. 3, p. 331.

dency to write more or less automatically; the second group (A, D, S and Y) a strong tendency to control writing consciously. Distraction and change in direction of attention threw such tendencies into the foreground. The results from the members of the same group are, in general, comparable from the standpoint of method used in enforcing attention; number and character of lapses resulting from distraction; variation in rapidity of writing under distraction; and variation in size of writing under distraction.

A glance at the conclusion of Part I (p. 51 f.) will show that the grouping of the reagents that resulted from the experiments reported in that section of the paper parallels that given in the above paragraph as a result of the tests of Part II.

PART III.

The reports from certain subjects in the tests just described raised the question as to how far the words of the experimenter, who gave the directions what to write, could constitute the writing-cue, particularly in the case of the test on the name or phrase. Again, reagents sometimes spoke of the hand being 'set' to write the given phrase previous to the signal.

The tests now to be described (1) were planned, therefore, for the purpose of testing (*a*) the result on the cue when the signal for writing was delayed; (*b*) the length of time the 'set' described above could last.

Another set of experiments (2) was also planned in order to test further the nature of the different imagery processes and the possibility of pure grapho-motor imagery and to determine the effect of doubling ideational processes apart from the act of writing.

1. Test in Which There was a Time-Lapse Between the Giving of Directions What to Write and the Signal for Writing.

During this time the subject counted aloud by varied intervals.

The series was planned with the purpose of testing the effect of a delayed signal on the initiation of writing. The following questions, particularly, were in mind:

1. What variation in result will occur as the time during which writing is delayed is increased. For instance, will the experimental directions what to write be effective as cue if the time-interval is short, but insufficient as the interval is lengthened beyond a certain point? If so, the effectiveness of the experimental directions to serve as cue should be held in mind, if one would plan a method to test the non-sensory initiation of voluntary movement. Again, is the prolonging of the cue-effect of the experimental directions, if such is found, due to

the production of a 'nervous set' in the writing musculature? During the longer intervals will there be a recurrence of the feeling of 'set' and will this be preceded by the recurrence of a sensory cue of some sort?

2. What difference in results will be effected by variations in the difficulty of the interval used in counting, counting by 13's for instance in contrast with counting by 2's? As the attention is fully occupied by the counting operation, how will consciousness of the phrase to be written be retained? Will it be possible at any point to get hold of a meaning-consciousness that is not in any way sensory?

3. What will be the effect of change in the directions what to write, after the subject has been habituated to writing some particular phrase under the conditions?

Method.—Before attempting to answer these questions, a brief summary of method and results may be given. The subject was told what to write when a second signal should be given. At an initial signal he was instructed to begin counting by given intervals varying in difficulty from 1 to 13 (counting by 1, 2, 3, 5, 10 and 11 was considered easy; by 4, 6, 7, 8, 9, 12 and 13 difficult). The time given to counting before the writing began varied from 1 to 60 seconds. In the majority of the tests the subject wrote his own name; but at certain points in the test, the phrase 'University of Chicago,' or some other simple phrase was substituted for the name. The writing of the name was chosen as test, since, being so automatic an act, any conclusions as to the need of sensory control based on results so obtained would, *a fortiori*, probably hold in other cases. Most of the tests were tried with the subjects blindfolded, so as to shut out visual sensory stimuli as motor cues. A sufficient number of experiments to show that the results are not materially different when the eyes are open was, however, tried. At signal, hand and pen were in position ready to write, but the pen was not in contact with the paper.

Result.—Total number of tests, 146.

Number of times in which there was reported a distinct initial cue at second signal, 102.

Number of times in which the second signal was reported as initiative of writing, 29.

Number of times in which no cue was reported or there was hesitation or difficulty in describing cue, 15. (The writer has included here all reports of 'hand set' as producing movement, and all cases involving introspective difficulty.)

It was the intention of the writer to tabulate the results with reference to the number of times a cue was present in the long and short time intervals and with the easy or difficult counting-intervals, but so great was the individual variation that such a numerical summing up of results had little meaning. The following tabulation of results for the individual subjects makes this evident.

Subject.	Whole Number of Tests.	Number of Times Initial Cue.	Number of Times Signal was Cue.	Number of Times no Cue or ? Cue.
A.	21	16	3	2
S.	18	18	0	0
R.	26	15	7	4
W.	10	7	2	1
Y.	14	10	3	1
D.	33	25	1	7
H.	16	6	10	0
B.	8	5	3	0

It is obvious that had several subjects been of the same type as H the outcome, taken numerically, would have been very different. On the other hand, had more subjects resembled S the preponderance in favor of an initiatory cue would have been even greater, for in the case of S every act of writing was initiated by a distinct visual cue.

Put descriptively the different experimental conditions result as follows. *If the counting operation is easy,* both long and short time-intervals are usually filled with continuous writing-imagery or such images keep recurring after blanks; the imagery is, usually, persistent if the time interval is a short one (R, B, A and S). Whether or not the second signal and recurrent image coincide in time is more or less a matter of chance; for the recurrent cues produce a 'nervous set' of the musculature which dies out only gradually (R and B). For D, H and W counting by the easy intervals was productive of little imagery. D required a distinct cue at the moment of second signal. Sensory material in the shape of visualized

hand and pen was sometimes present for D under such conditions of counting, but such sensory material was felt to be inadequate as cue.

When the counting operation is *difficult*, there is less wealth of imagery. During *long* intervals there is apt to be recurrence of sensory cue which fades and revives; if the time-interval is *short*, the subject reports usually that the signal set off the writing, or that the hand was 'set' for the writing in consequence of a 'cue' got during some break in the counting (B, R and Y); or, again, there is a pause at the close of counting followed by a distinct initiatory cue (B, R, Y and D). S and A had little imagery during counting and at second signal initiated writing with a distinct sensory cue.

Such a summary answers several of the questions raised above. The delay of writing during a period of time that is filled with a more or less difficult mental operation results in a large amount of sensory material that is probably to a certain extent superfluous. The lengthening of the time between signal and signal necessitates a recurrence of cue during counting, or the getting of a distinct cue at the moment of writing. Both the experimental directions and the presence of writing-cue produce a 'set' in the writing musculature which persists perceptibly and, introspectively, can be felt waning. Conditions that favor its recognition are (1) a short time-lapse between signals, this interval being filled with an operation that engrosses attention; or (2) a long time-lapse under the same conditions, when the blanks between the recurrent writing-cues will be filled to a certain extent with the feeling of 'writing-set.' There are, however, individual differences in the frequency with which attention is called to this feeling of incipient innervation. Where the interval between signal and signal was unusually long, subjects sometimes reported muscular fatigue of the hand (R and D). In several instances, the innervation was effective only for the first one or two letters of the phrase and after writing so much, the subject would resort to an articulatory or visual cue.

The test furnished absolutely no introspective evidence for a non-sensory meaning-consciousness. It is true that as ex-

perimental evidence there might be cited the cases in which the second signal was reported as initiating the movement. But these cases are comparatively few in number and the conditions under which they occur usually favor a different interpretation. In three cases (R) the signal came after a long delay during which time writing-cues had occurred again and again. In four other cases (A and Y) the response was a reflex one, as shown by the fact that the subject wrote his name and *not* the phrase that he had been instructed to write. Moreover, the fact that H contributes over a third of the cases under consideration is highly instructive; for H's responses in this, as in other tests, were very automatic, shown in this instance by the jerk of the hand at the signal, with the introspective report that the first part, or the whole, of the given phrase was written before the subject was aware what she was doing; and by the further fact that when the experimenter changed the directions, the signal would at first call out the old movement.

So little evidence in fact is there for a non-sensory consciousness, that when directed to write some of the less usual phrases, the subject had recourse to sensory enforcement in order to maintain consciousness of experimental directions. S, for instance, added to his usual visual cue an articulatory one; R surprised herself by saying *aloud* the phrase she was to write; H made use of visual images—an unusual performance.

An answer to the question as to the result, when the phrase to be written is changed, after the subject had become habituated to writing it, has already been anticipated. Such a change from name to phrase usually caused a momentary conflict, with an inhibition of the original cue or movement. The case of S furnished an exception to this statement, for he showed no tendency to respond in any reflex manner. The change in instructions was evident in his case only by a slightly longer pause before beginning to write and, sometimes, by an enforcement of the cue. The other subjects show differences in the method by which the habitual response was inhibited. H, for instance, would write the first letter or part of the first letter of her name, and only become conscious of her error through a kinæsthetic report of the writing of these letters. After writing

the new phrase two or three times, under such conditions, these initial strokes became her cue for writing the new phrase. For A consciousness of error came through visual report on the letter made.

Y twice wrote his whole name at the giving of the second signal without any consciousness of error, until his attention was called to the fact; another time he became conscious of the error after the first stroke was made; and a fourth time merely inhibited the name-cue (articulatory). The first two errors are of particular interest and probably indicate not a reflex response, but an inadequate cue; for Y, who had previously been wholly at a loss when the signal came, had in these two experiments deliberately attempted to widen the field of consciousness during counting, so as to include consciousness of the hand. The hand, visualized or perceived through sensations of contact or innervation, had been attended to during the interval that elapsed between signals. Such a hand-consciousness was adequate to produce movement at the signal, but not sufficient to initiate the writing required.

At times, the subject instead of responding to the signal with a movement, reacted with an articulatory or visual cue, the inhibition of which was necessary. For R, B, H and A, there was a tendency to respond with a movement; D usually reacted with a name-cue.

Such differences in reaction mark, again, the varying degrees of automatism. In this connection the reflex arousal of an habitual sensory cue raises the question as to the possible meaning of sensory automatism.

2. Description and Timing of Various Possible Writing 'Controls' for the Verse, With and Without Distraction, and Apart from Actual Writing.

The object of such a test was (1) to get material for comparing the time required for manipulating any particular control process and for actually writing the verse; (2) to determine the degree to which any particular process could be slurred and still carry meaning and be 'felt' to constitute an adequate

motor cue; (3) to test, further, individual facility relative to the use of certain prescribed processes.

The following time-readings were obtained for seven subjects. The number of readings reported under each heading, indicates the number of tests.

SUBJECT R.

Overt articulation of verse: 6.9s., 4s., 3.4s. (slurred), 3.8s. (slurred).

Incipient articulation of verse: 3.9s., 4.6s., 3.8s., 2.8s. (slurred), 2.9s. (slurred).

Grapho-motor imaging of verse: 28s., 66s., 65s.

Overt articulation of verse, accompanied by mental counting (auditory-vocal-motor): 7.9s., C. 10;[1] 5.9s., C. 12; 5.2s., C. 5.

Imaging of verse (visual) while counting aloud: 6.8s., C. 31; same, verse mentally articulated, 7.8s., C. 31.

SUBJECT A.

Overt articulation of verse: 7.5s.

Incipient articulation of verse: 5s.

Grapho-motor imaging of verse: 25s. (see description below), 167s.

Overt articulation of verse accompanied by mental counting (articulatory): 16s., C. 10.

Imaging of verse (auditory) while counting aloud: 66s., C. 186.

Imaging verse (articulatory) while counting mentally (visual): 35s., C. 21.

Imaging verse (auditory-articulatory) while counting mentally (auditory-articulatory): 25s., C. 16.

SUBJECT D.

Overt articulation of verse, 7.2s.

Incipient articulation of verse: 4s., 4.6s., 3s., 3.4s., 3.2s. (slurred), 3.8s., 3.6s. (slurred).

Grapho-motor imaging of verse: 192s.

Overt articulation of verse accompanied by mental counting (visual): 12.6s., C. 15.

Imaging of verse (auditory-articulatory) while counting aloud: 20.2s., C. 48; 19.2s., C. 44.

Imaging verse (articulatory) while counting mentally (auditory): 16.6s.; C. 12; 16.6s., C. 16.

SUBJECT B.

Overt articulation of verse: 9s.

Incipient articulation of verse: 7.8s.

Overt articulation of verse while counting mentally (auditory-articulatory): 21.6s., C. 43.

Imaging of verse (auditory) while counting aloud: 52.6s., C. —; 53.4s., C. 69.

Imaging verse (auditory) while counting mentally (auditory): 18.4s., C. 14.

Grapho-motor imaging of verse: —.

SUBJECT Y.

Overt articulation of verse: 8.4s., 6.9s.

Incipient articulation of verse: 10s., 10.3s. (slurred), 12.7s., 7s. (effort increased, some visual accompaniment).

[1] C. indicates number counted.

Grapho-motor imaging of verse: 91s., over 80s.
Overt articulation of verse while counting mentally (visual): 13s., C. 13.
Imaging of verse (visual) while counting aloud: 22s., C. 29.
Imaging verse (articulatory) while counting mentally (visual): 15s., C. 20.

SUBJECT W.

Overt articulation of verse: 11.2s.
Incipient articulation of verse: 5.4s., 5.2s. (slurred).
Grapho-motor imaging of verse: 34s., 35s.
Overt articulation of verse and mental counting (articulatory): 13.7s., C. 24.
Imaging of verse (slurred articulation) and counting aloud: 12s., C. 14.
Imaging verse (articulatory) and counting mentally (visual): 11s.

SUBJECT S.

Overt articulation of verse, 9s.
Incipient articulation of verse: 7.8s., 5s. (articulation clipped).
Visualizing of verse: 9.4s.
Grapho-motor imaging of verse: 80s. (see notes below), 93s., 93s.
Overt articulation of verse while counting mentally (articulatory): 15.6s., C. 7.
Imaging of verse (blurred articulation) while counting aloud: 31s., C. 55.
Imaging of verse (articulatory) while counting mentally (partly visual): 31.6s., C. 28.

It is evident, with the exception in several cases of tests on grapho-motor imagery, that the time required for the different methods of imaging the verse falls far within the time required for writing it. Therefore Mr. Burnett's[2] criticism against the classical theory of volition on the score that the time needed to obtain a series of adequate motor cues exceeds that required for execution of the intended movement cannot be urged against the efficiency of the ideational cue in writing, at least.

Moreover, even with a distracting or accompanying mental

[2] C. T. Burnett: 'An Experimental Test of the Classical Theory of Volition,' *Studies in Philosophy and Psychology*, pp. 392–401. An unfortunate assumption vitiates Mr. Burnett's method of testing the so-called classical theory of volition; for what theory, however classical (!), would maintain that to a series of repeated movements, a series of cues must correspond, point for point and with quantitative fidelity? It would be more to the point to determine the rapidity with which one could ideate a series (granting a series be necessary) of cues, which, however much slurred, felt adequate to the purpose. Of course in a series of movements such as were considered by Mr. Burnett, a series of anticipatory ideational cues would be unnecessary. The process once initiated, no cue would be needed other than the kinæsthetic or other report from the member moved. The real question at issue would seem to be the necessity in every case of an *initial sensory* cue of some sort.

process, the mental cue series falls, temporally considered, far within the limits required for writing. Certain subjects who found writing under distraction exceedingly difficult, were able to maintain the double mental process with little effort. This result indicates, again, the degree to which awareness, in kinæsthetic or other terms, of the movement executed enters consciousness—to complicate the situation in distracting tests, but, normally, to serve as a control process.

The degree to which a process could be slurred or telescoped and still symbolize the original meaning was imperfectly worked out. In reporting upon suppressed articulation of the verse many subjects called attention to the blurring of the process, and the suggestion on the part of the experimenter that the process be hurried as much as possible resulted in clipping mental articulation, and blurring mental pronunciation. Such slurring of mental, or even of overt, articulation involved articulating only emphatic words or syllables, while between the words thus articulated, intervened muscular tensions, tensions which had meaning only in the sequence taken as a whole. No doubt the maximum possible reduction of such sensory content would issue in a state of consciousness, that approached introspectively from the other end, that is when achieved, would seem to be a pure meaning consciousness, altogether devoid of sensory content.

It was impossible to continue this series of tests to the furthest point of profit. This was the more unfortunate in that the method suggests a way of getting more light upon the relation of auditory to articulatory verbal images, since the slurring of the two processes was by no means correspondent in all cases.

The unequal facility with which the subjects executed the experimental directions in this test is shown by the differences in speed. Again, individual differences were manifest in the manner in which any particular distraction process was dealt with; for in the case of the second, or distracting, process the subject was unconstrained by directions. In so far as the individual variations are merely confirmatory of those observed in the distraction experiments in general, there would be no

value in a second rehearsal of them. The following new observations are of interest. Three subjects (W, R and Y) during this series of experiments laid stress upon the variations in tension in the musculature of the ear, observed during mental articulation. W was inclined to believe that all auditory toning of mental articulation was, in his experience, of such pseudo-auditory quality. Again, a curious variation in the matter of counting-consciousness was brought out in the tests. No slurring of mental counting was possible for W and D, although both were able to slur the verse-pronunciation to a considerable degree without loss of meaning. In fact, counting *mentally*, on the whole, was felt to possess little meaning; both resort, in addition of numbers, to overt articulation, when this is possible. R, D and W report that counting to have meaning must involve consecutive articulation (without slurring); while A, on the contrary, reports that verse-articulation to have meaning must be continuous, while counting may retain meaning though discontinuous.

The most interesting and characteristic results were those obtained in the grapho-motor imaging of the verse. The mode of response in this test at once marked the subject as 'sensory' or 'motor' and confirmed the conclusions reached as a result of the other experiments. A, D, Y and S brought the 'hand' into consciousness only with effort. Actually to imagine writing movement was for these subjects a much more laborious process and one requiring more time to carry out than would overt writing. The natural tendency on the part of these subjects was to image the writing-act in visual or articulatory terms, not hand-kinæsthetic terms, the movement quality being obtained through the serial progression of the images. For R and W, on the other hand, consciousness was preoccupied with kinæsthetic images and sensations and the time needed to image the verse in grapho-motor form was below or approximated that actually taken for writing. Unfortunately, records as to the grapho-motor imagery of B and H were not obtained (both of whom were shown by other tests to be strongly hand-motor). A test on B in another connection (cited below) indicates the way he would, in all likelihood, have responded to the test.

The introspective reports from the different subjects are given below; the details are felt to be instructive.

SUBJECT R.

First Trial.—Hand in lap. It jerked frequently, particularly, R reported, at end of lines. There was actual innervation of the hand and R was not sure that there was any hand-kinæsthetic material other than such sensational-awareness. Besides feeling the hand move, R could see it move. There was, moreover, an indistinct visual report, word by word, of the writing, and complete articulation and spelling out of the longer words. The paper on which the writing was seen was not in the same locality as the hand; it was on the table, tilted at usual angle. Time, 28s.

Second Trial.—Eyes closed, hand resting on table, first finger extended. Tremor in whole arm and finger; actual innervation at difficult places, punctuation places, for instance. Throughout, strong feeling of movement and of inhibition of movement; arm had weak, 'invalid' feeling. Auditory-articulatory accompaniment more laborious and distinct than during actual writing. Visual report, word by word, hazy on grey background. Time, 66s.

Third Trial.—Eyes closed. Hand, grasping pencil firmly, rested on paper laid on table. Contact sensations restricted consciousness of movement. No i's could be dotted or t's crossed because the pencil could not be raised. Area of movement restricted; words close together and all joined; no lining, each line joined last word of preceding line. Paper was shifted from under pencil. But little feeling of movement inhibition. Slight feeling of hand-innervation at beginning of verse; toward close kinæsthetic *imagery* emerged. Time, 66s.

SUBJECT A.

First Trial.—No consciousness of hand. Writing pace was that of reading. Verse was syllabified and seen in own hand-writing, close resemblance to visual report of writing. Letters came in serial groups. Eye movement across lines, probably an actual movement. Time, 25s.

Second Trial.—A instructed to force graphic imagery into hand-terms, if possible. Eyes were closed, pen held in the hand, and hand, but not pen, was in contact with paper. Experiment very difficult. Writing was done from auditory and visual cues, anticipatory. These cues came much more rapidly than the hand could take care of them. There was no image of hand or pen, but there was a slow and laborious visual report *letter by letter,* with some movement-feeling, probably due to actual innervation of hand. There was also eye-movement accompanying the tracing of letters. Time, 167s.

SUBJECT D.

First Trial.—Eyes closed, hand resting on table. There was little consciousness of hand except at places where D deliberately attempted to get grapho-motor imagery by bringing hand into the focus of attention. At the beginning of the test, however, no motor imagery was involved. Words were visualized and articulated, letter by letter, with accompanying eye-movement, and movement in tip of tongue (probably actual innervation). Again, hand would be

visualized and run over contour of letters. This visualized hand bore no particular relation to actual hand. Sometimes the visualizing seemed to precede attempted hand-movement, at other times to follow as a report. It was very difficult work. Only distinct hand-motor image came in connection with a capital 'J,' an initial of D's name which is normally executed with a big sweeping movement. Toward the middle of the verse, grapho-motor imagery began to come with less effort. It had a peculiar quality in that there was a striking and distressing sense of the absence of resistance. The experience was like writing in the air or on a yielding surface. Time, 192s.

SUBJECT B.

First Trial.—Eyes closed. Hand clasping pen but pen not in contact with paper. B was instructed to get hand-kinæsthetic imagery involved in writing of name. There was a wealth of tactile, kinæsthetic, and visual images. Effort was needed to hold the real pen still, in order to keep the visualized pen from dancing too energetically.

SUBJECT Y.

First Trial.—Eyes closed, pen held in hand, both in contact with paper. Little consciousness of hand except when it moved slightly. Y ran eyes over the contours of the letters. Y saw the letters as they made themselves but there was little feeling of agency. There was double imagery process, visual and articulatory. The two processes did not keep together, different words would be made by the two methods at different times. Perhaps the visual material was in the nature of a report. Time, 91s.

Second Trial.—Arms clasped. Y was unable to get hand into consciousness. Contact sensations absent in this test reported to be important part of writing-consciousness. Eyes were run over the contour of visualized letters in the attempt to get movement. Y stopped before completing verse. Time, 80s.

SUBJECT W.

First Trial.—Eyes closed. Pen clasped in the hand but not in contact with paper. W experienced an almost irresistible tendency to make actual movement. When this tendency was not present there was mere articulation of verse and no writing consciousness. Perhaps there was a dim visual report for certain letters. Time, 34s.

Second Trial.—Hand held behind back without pen, fingers open, palm out. Fingers moved frequently. Actual innervation. Hand was visualized as stationary but 'felt' moving. The failure to get normal report from writing movements would cause visualizing of hand and perhaps a drifting into a merely articulatory consciousness of verse. Time, 35s.

SUBJECT S.

First Trial.—Eyes closed. Pen in hand and in contact with paper. The test was difficult. Articulation of verse, word by word. When S went slowly, got innervation feeling from arm. To get this, necessary to check articulation speed. Visual report, letter by letter, as if writing. Conscious of hand, but little feeling in it. No eye-movement noticed; letters appeared in place on paper. Time, 80s.

Second Trial.—Innervation in hand actually felt, particularly in line and word-spacing. Inhibition necessary in order to keep from writing. Articulation throughout. Visual report only at times when looked for. Attention strongly on hand. Each time the hand-movement was inhibited, visual image of hand came to focus of attention. Time, 93s.

Third Trial.—Eyes closed. Hand held behind back without pen. Very little feeling of innervation. Principal imagery obtained was a *letter by letter* visual report after articulation. Time, 93s.

3. Conclusion, Part III.

The results from the experiments reported in Part III confirm those reached in Parts I and II. Those reagents shown by the previous tests to be particularly dependent upon grapho-motor processes, in the tests just described respond more reflexly to a delayed signal and show, also, greater facility in the ideating of a grapho-motor process than do the other subjects. The evidence they afford for pure grapho-motor imagery is, however, ambiguous since innervation of the writing musculature was evidently present in many instances. Perhaps the best evidence of grapho-motor imagery occurred in the case of D, who with effort succeeded in getting at intervals such imagery quite divorced from sensational consciousness of the hand.

The tests gave no evidence for a non-sensory initiation of writing and showed that in the case of the writing act the time required for ideating a series of writing-cues fell far within the time required for writing the verse. The only exception to this statement was found in the ideating of a *grapho-motor* series of cues by S, Y, D and A.

That to a certain extent the extreme difficulty experienced by Y in the distraction tests was due to innervation difficulties and not to the maintaining of a double mental process, was shown by the ease with which he maintained the two processes when not required to write while so doing.

Part IV. Summary and General Conclusion.

1. *Comparison of Results. Parts I, II and III.*

It remains to bring together briefly the results of the tests cited in the different parts of the paper. Part I attempted to throw into relief the control processes utilized in writing by eliminating or embarrassing some particular control. Thus it tested the effect of elimination of the seen result of writing by blindfolding the subject; the embarrassment of the motor control through changes in the grapho-motor report introduced by requiring the reagent to write with the left hand; the embarrassment of both the visual and the grapho-motor 'controls' by demanding of the subject inverted and mirror-writing and writing under some strained position of hand and arm. These experiments made evident that some reagents (B, R, H and F) were much less embarrassed by change in the visual factors of the situation than were D, A, S and Y. Moreover, the control in the case of the former group of subjects proved to be much less conscious than that of the latter. When the break-up in the motor coördination was sufficient to demand the acquiring of a new writing-reaction, the former reagents tended to use consciously more motor material than did the latter, for whom the break-up usually occasioned an intensification of the visual control. Quickness in adaptation to the new requirements, when the break-up was considerable, was, however, not wholly dependent upon the sort of imagery used, but was a function of other factors not determined. Only so long as the grapho-motor habit permitted an automatic response, were the reagents of the first group as a whole superior to those of the second. The limits within which the writing automatism could function differed considerably in individuals of the first group.

In the distraction experiments of Part II the problem was no longer one of the method used in acquiring a novel writing

habit, or of the limits within which the old writing habit could function, but a question of the method by which a double process could be maintained. The reagents fell, however, into exactly the same groups as before. Distraction with those of the first group caused a lapse of writing consciousness, so that the writing went on automatically or with control at intervals. The accuracy of the writing under such conditions depended upon the specific conditions. If the distraction were particularly effective, accurate writing could often be maintained for several seconds at a time. An intermittent distraction allowed oscillation of attention which was productive of errors. With the reagents of the second group, for whom writing was less automatized, loss of writing-consciousness occurred rarely. Writing was consciously controlled. Distraction did not, in reality, take place at all; either there was deliberate and constant oscillation of attention, or else the reagents succeeded in synchronizing the two processes carried on. When successful, the latter process was an effective but laborious way of meeting the situation. The attempt to throw attention upon different points in the situation emphasized the grouping of subjects already cited. The first group gave attention to writing only with effort and usually resorted to some indirect means, such as the vocal-motor, for enforcing the writing consciousness. The second group, whose attention focused on writing naturally, concentrated on some more intimate factor of the writing act, such as the visual report or the grapho-motor sensations.

A further complication was introduced by the fact that for some reagents the distracting process appealed to automatized habits. A working out of the control processes in a different act, say that of speaking, would probably cause a re-grouping of the subjects.

Some interesting facts as to the nature and functioning of the different imagery processes were obtained from the distraction experiments. The individual records give these in full.

Part III confirmed the conclusions of Parts I and II. The reflex response to a signal, when the response was delayed and the interval between the giving of the experimental directions and the signal for writing was mentally occupied, emphasized

the automatic character of the reaction of the first group. The members of the other group required a new cue before beginning to write. The subjects of the first group, again, were able when required to do so, to get grapho-motor imagery, although results were complicated by the hand-innervation which resulted from thinking about the process of writing. The other group ideated the act of writing in visual and vocal-motor terms and, unless forced to do so, did not bring hand-movement into the situation.

The agreement in the results of the several tests is striking, when the possibility of error in introspection and of failure to follow experimental directions is considered. The agreement under such varied conditions should weaken any criticism that would discount the results as the forced products of experimental conditions.

The bearing of the tests upon the questions raised in the introduction to the paper has been emphasized in the course of the discussion. To recapitulate briefly: (1) The experiments offer some evidence for the existence of grapho-motor imagery, but this evidence is not unambiguous. (2) Certain results suggest the possibility of complete dissociation of auditory and vocal-motor imagery; but, again, the evidence is not conclusive. (3) No evidence was found for the initiation of a *voluntary* act of writing without a sensory cue of some sort. (4) Throughout the whole series of experiments, the *report* coming from the writing in terms either of kinæsthetic or visual sensations and images, proved to have a highly important function as part of the writing-cue. (5) When automatic writing occurred, it was, apparently, purely physiological in character. (6) Pen errors frequently resulted from a lapsing of the grapho-motor report from the writing and were most often found in reagents of the first group. (7) Complete distraction of attention for any length of time occurred but rarely. The production of such a complete distraction for any one individual demands consideration of the attentional value of the different sense processes for that individual.

2. *Mental Types.*

As to the bearing of the individual differences brought out in the tests upon general mental types but little can be said. The main difference between subjects was the extent to which writing was automatized and the degree to which the reagent allowed conscious direction to lapse. It would be interesting if one might cite this as a general tendency on the part of each particular reagent, and label the individual subjects as 'sensory' or 'motor,' using these terms as Baldwin uses them in his discussion of mental types.[1] It is obvious, however, that such a conclusion could be drawn only after a test of the same subjects in various different acts. It does not even follow that the imagery control would remain constant for any one individual if the act tested were changed. A shift from a verbal to a concrete situation might well cause a shift in method of control. To a certain extent, however, the results seem to have a general significance. Y and S both report slight tendency to automatism in general. They do but one thing at a time, and give careful supervision to their actions. On the other hand, H and R report that their actions become well automatized. Absent-mindedness occurs frequently in the case of H.

Again, the question of quickness in adaptation to new conditions is interesting. R., whose ease in meeting the requirements of the experiments was particularly noticeable, possesses also great adaptability in general. D. was slow in adapting herself to the new conditions. This is also a general characteristic.

3. *Speed, Mean Variation and Appearance of Writing.*

The number of subjects was too small to permit drawing any conclusions as to the relation between the writing control and the appearance and speed of writing. Yet the following facts are of interest. A, whose control was visual and often anticipatory, was by far the most rapid writer of the nine tested. B (motor) came second. S (visual in control) gave the lowest mean variation in speed; B the highest. The relative position of these two subjects remained the same whether the mean variation in speed was obtained from the different trials in

[1] Baldwin: 'The Story of the Mind,' Chap. VIII.

writing the verse on different days, or got from the series of fifty autographs written the same day.

Undoubtedly some correlation exists between the appearance of any particular individual's handwriting and his method of control. But tests on such a small number of subjects can only be suggestive. In general, those subjects strongly dependent upon grapho-motor sensational control wrote a more flowing, a freer hand than did the other subjects. A tendency toward diminution in the size of characters was most noticeable among subjects of the second group. So far as excellence of penmanship is concerned probably S and R would be cited as producing the most effective results. S. was often preoccupied with a visual copy which he consciously (and conscientiously) imitated. Clerical work has emphasized this tendency. R, who was strongly dependent upon grapho-motor control, wrote a more flowing and more individual hand, slightly more rapid than that of S but more variable both in speed and appearance. A, who relied to a considerable degree upon visual anticipation, was apt to slur the formation of letters, particularly after a word was once safely initiated. There is apparent throughout impatience at being delayed by the necessity of writing at all. H's writing shows a blurring of details, and often exhibits a tendency to graphic stammering. B (control grapho-motor) wrote a rapid and perfectly unambitious hand that gave no evidence of visual elaboration. The writing of both Y and D improved in quality when visual supervision was given it.

Again, the number of reagents was too small to give much significance to a grouping based on sex. Of the nine subjects tested four were women; five, men. Three of the women and two men constituted the first group of subjects, who were dependent upon grapho-motor sensational control. One woman and three men composed the other group, who gave visual attention to writing. The proportion in the two groups is not in accordance with Gesell's[2] conclusion that girls (and women) are in writing, more preoccupied with a visual standard than are boys (and men). Diehl[3] found that women wrote a larger

[2] 'Accuracy in Handwriting as Related to School Intelligence and Sex,' *Am. Jour. of Psychol.*, Vol. XVII., p. 394.

[3] *Locus citus*, p. 61.

hand than men, wrote more rapidly and with greater ease—a description which sounds like dependence upon grapho-motor control. But in the present series, the most noticeably small copy was written by a woman.

A detailed test of a large number of individuals would undoubtedly yield characteristic results as to the relation between writing-control and sex, and writing-control and general appearance of penmanship.

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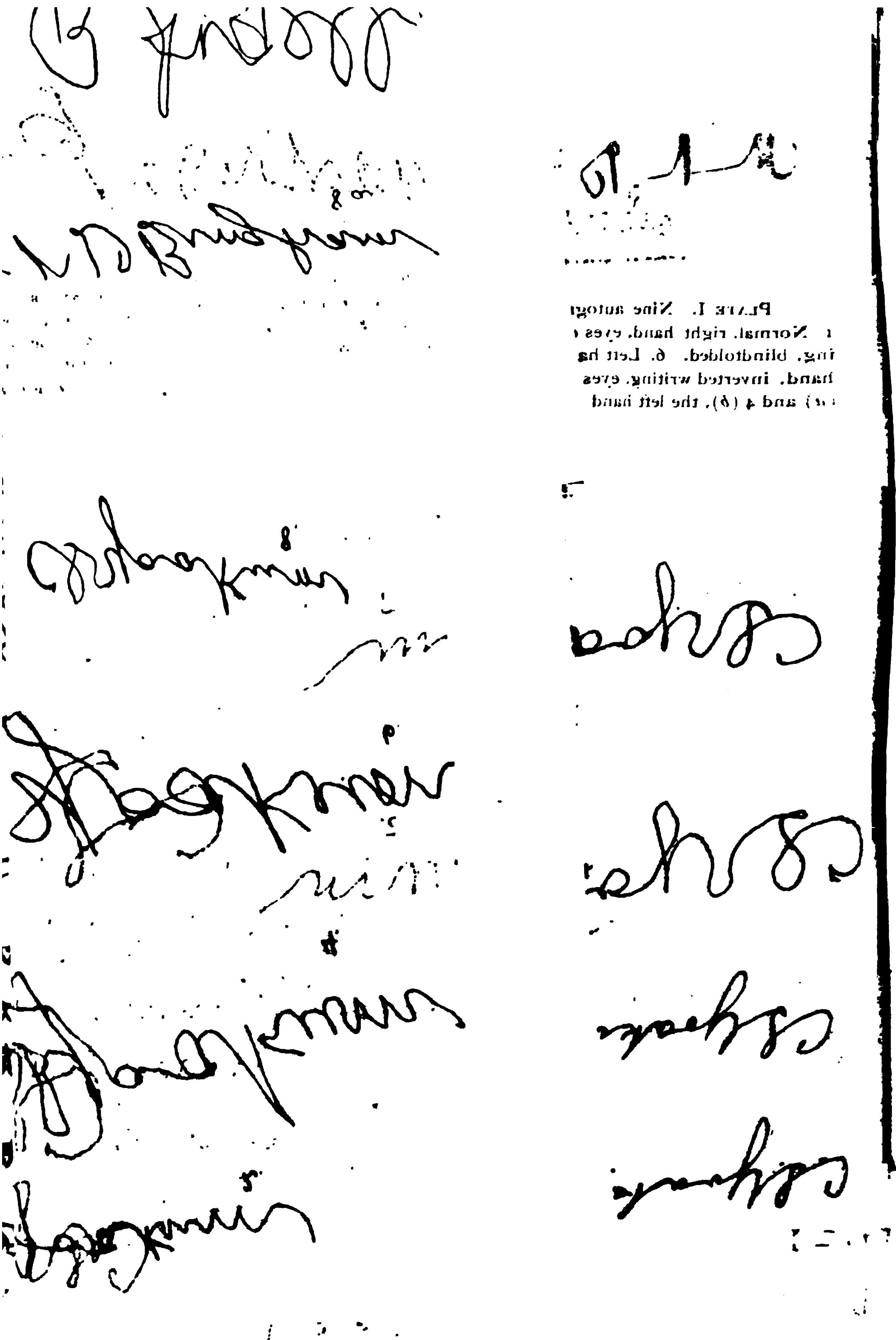
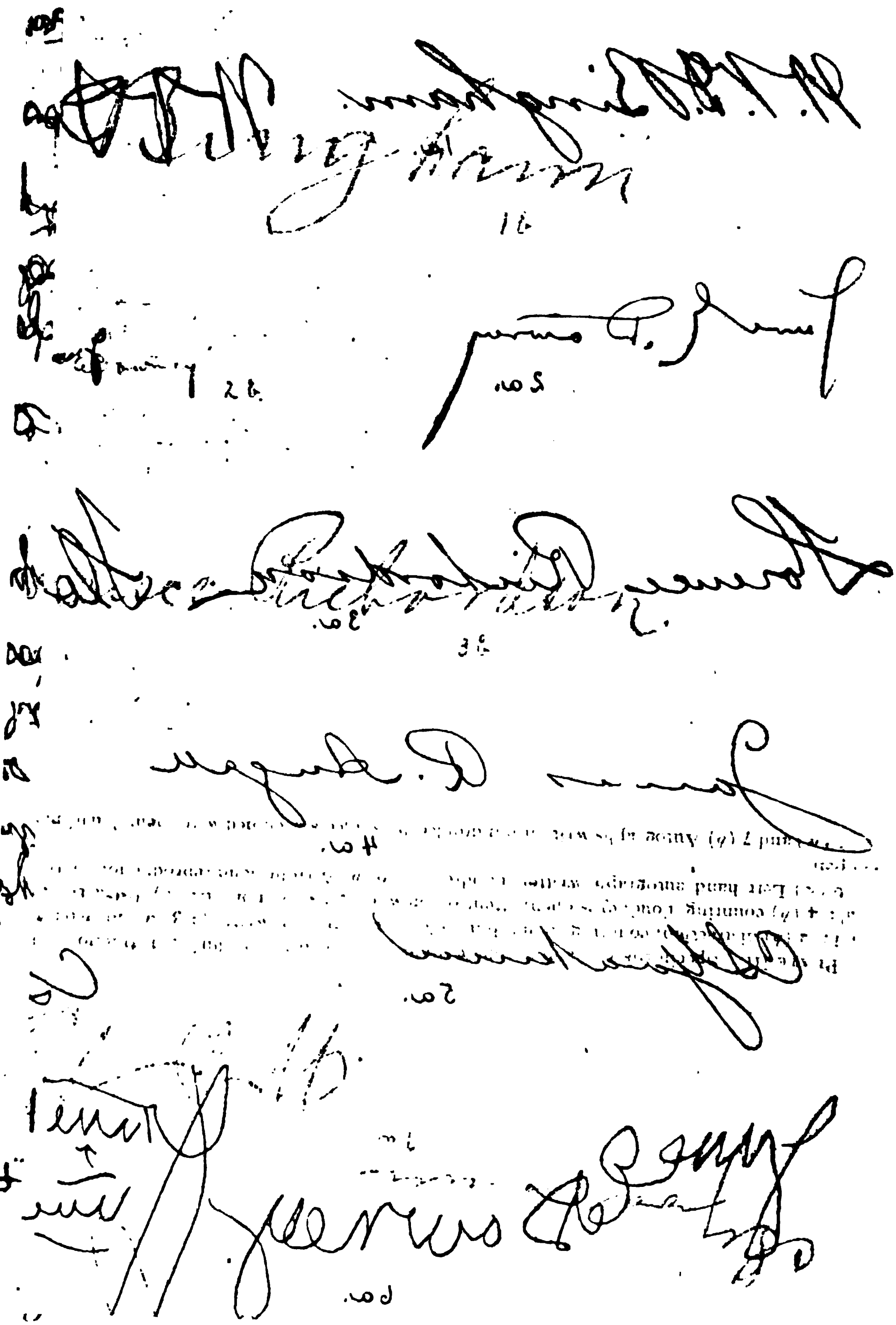


PLATE I. Nine autogr
1 Normal, right hand, eyes o
ing, blindfolded. 6. Left ha
hand. Inverted writing, eyes
(a) and 4 (b), the left hand

PLATE III



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June, 1908
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Psychological Review

University of Iowa Studies in Psychology

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CARL EMIL SEASHORE

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PERIMETRY OF THE LOCALIZATION OF SOUND.

BY

DANIEL STARCH, A.M., PH.D.

PART II.

CONTENTS.

The first part of this investigation was published under the same title in *The University of Iowa Studies in Psychology*, 1905, IV, 1–45. The object of Part I was to determine the accuracy of localization in various representative directions. The present research is an effort to study in detail some of the numerous problems to which those experiments gave rise. The following is a report of five series of experiments which were made during the academic years 1904–06.[1]

SERIES I.

Sensibility to Sound in Different Directions.

Problem, Method, Apparatus and Observers.

One of the most frequently recurring observations in the experiments of Part I was the fact that a sound of uniform intensity and at a constant distance from the observer seemed to be nearer and correspondingly louder in some directions than in others. The problem then was to determine the extent of this variation in apparent intensity and apparent distance of an objectively uniform sound.

How can this subjective variation be measured in a definite way? Intensity of sound varies in some ratio, inversely with its distance; we estimate the distance of a sound by its intensity and the intensity is interpreted partly in terms of distance. We want to measure, in a selected group of directions, the actual variation in intensity that is necessary to make the sound seem uniform to an observer. Assuming that the sensibility or threshold of hearing remains fairly constant during the time of the measurements, we may first measure the acuity of hearing in representative directions in which apparent changes of intensity have been noticed. The differences in the thresholds of

[1] The writer wishes to express sincere gratitude to Prof. C. E. Seashore for suggesting the original problem in Part I, for his many suggestions of new problems and methods of experimentation throughout the entire investigation, and for the time he has given as observer in several series of experiments. Acknowledgments are also due to all the observers who participated in the experiments.

hearing among these directions may then be regarded as measures of the differences in apparent intensity.

For this purpose, apparatus was needed to produce a sound variable in intensity according to definite units but uniform in all other respects. An electric fork of 100 v. d. was driven by a current of three amperes and three volts, which were kept constant throughout the experiments. A shunt from the fork was completed through the primary coil of Seashore's audiometer,[1] of which the secondary coil was connected with a telephone receiver. The audiometer is an instrument devised for the purpose of controlling and measuring the intensity of sound. The essential part of the audiometer is a primary and a series of secondary coils by which the strength of the current and consequently the intensity of the sound can be varied. The scale of intensities which rises from one to forty, is based on the psychophysical law so that the ratio of any two successive increments on the scale is psychologically the same.

The apparatus was distributed in three rooms. The fork was mounted in the battery closet, the audiometer operated by the experimenter was in the measuring room, and the receiver was mounted in the observing room which was moderately lighted by incandescent lamps, and practically sound-proof—a condition necessary for successful experiments of this kind. The receiver was mounted on a tripod and, in order to avoid resonance, was insulated from the iron support by heavy felt. A pasteboard tube, six inches long and two inches in diameter, lined with felt cloth, was attached to the face of the receiver for the purpose of directing the sound toward the observer. Throughout the tests the receiver was kept in the same position and at a constant distance from the observer, namely one meter from the center of the head.

The observer was guided in keeping the proper position by a ring of wire suspended from above and hanging freely about the head. In finding and keeping the various positions the

[1] For the original description of the audiometer see, Seashore, "An Audiometer," Univ. of Iowa Studies in Psych., 1898, II, 158–163. A briefer description can be found in the Univ. of Iowa Studies in Psych., 1905, IV, 48–49.

observer turned to the desired position in each case and was guided by labels on the walls.

The threshold measurements were made in a series of directions in the right half of the horizontal plane through the aural axis at points 15° apart. The points were, 0° front, 15° right front, 30°rf, 45°rf, 60°rf, 75°rf, 90°r, 75° right back, 60°rb, 45°rb, 30°rb, 15°rb, and 0° b.[1]

In determining the threshold of hearing we may proceed in two ways, following the method of minimal change; we may begin either with a subliminal sound and increase it until it is just perceptible, or with a supraliminal sound and decrease it until it is just not perceptible. In the former case we get a determination (*To*, threshold over) just a little above the determination obtained in the latter case (*Tu*, threshold under). The average of the two is regarded as the threshold. For the present purpose, however, it is better to use only one of the determinations and thereby have the advantage of a simpler computation and evaluation of the results. *To* is preferable to *Tu* because it is easier to determine the appearance of sound than its disappearance.

The measurement proceeded as follows. The observer comfortably seated on a stool in the observing room, held in his hand a strap key which was connected with a sounder and battery in the measuring room. All communication between the observer and the experimenter was by means of signals through the sounder and the receiver. The experimenter began by giving a loud sound in the receiver as the signal for starting. After an intermission of two or three seconds he started at a subliminal point, usually from five to seven units below the threshold, and increased the sound at the rate of approximately one step a second until the observer heard it and responded by a tap on his key. The step on the audiometer scale at which the response occurred was then recorded. This constituted one determination. In the same manner ten determinations were made in succession for a given standard direction. In

[1] Cf. Fig. 2, page 6, in Part I.
In this system of designating directions 0° front is directly in front, 0° back is straight back, and 90° right is opposite the right ear.

order to eliminate from the results the possible disturbance coming from the change in position on the part of the observer, in passing from one direction to another, the first three measurements at each direction were not recorded. After ten records had been made in one direction the experimenter again gave a loud sound through the receiver. The observer then turned to the next position and signalled as soon as he was ready to begin again.

The observer also had the right to throw out of the record any trial in which he had anticipated or delayed his response or in which some other disturbance had occurred to invalidate the measurement. But this did not occur often when conditions were normal. The observer had to indicate immediately his desire to discard a measurement, by signal to the experimenter. A complete record consisted of twenty determinations in the double fatigue order for each one of the thirteen chosen directions. The time required for taking such a record was approximately forty-five minutes.

Records were obtained from eight persons, N. C., D. S., C. E. S., E. A. J., N. B., E. G. Q., R. W. S., and H. S. B. Two of these, N. C. and N. B., are women. C. E. S. and D. S. were experienced observers and the others were students in the technical laboratory course. After two or three records had been taken on each observer, it was noticed that they fell into two distinct types according as the threshold for front was higher or lower than for the back. In one type the threshold for front was higher than for the back, and in the other type the reverse was true. One representative of each type was chosen (N. C. and D. S.), and a series of ten records was obtained from each one of these two. In all, thirty-five records were obtained from the eight observers, making 700 determinations for each direction, or 9,100 in all. After these tests, N. C. and D. S. each made three more records using only one ear.

Before the regular tests were begun the threshold was found for each ear separately in the case of every observer, as a large difference between the two ears might affect the records. An observer never made more than one record a day. They were at the same hour on successive days, barring a few exceptions.

The order of the directions was reversed for successive records; instead of beginning all the records in front, each alternate one was begun in the back so as to make the conditions as uniform as possible for all directions. The observer was alone in the room, which was thoroughly ventilated before each test.

Binaural Threshold.

Method of recording—The following is a typical record in the statistical form in which they were originally made during the course of the experiments. The numbers are the readings on the audiometer scale for the separate determinations.

Specimen Record.

	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
	22	20	18	16	14	14	14	13	12	12	13	14	15
	22	20	17	16	14	13	13	12	14	13	14	14	15
	19	20	17	16	14	14	12	12	12	13	15	14	15
	21	19	16	16	15	13	12	12	14	13	15	15	15
	20	19	15	15	15	12	13	13	13	13	14	15	14
	21	19	17	16	15	13	12	12	13	13	13	15	15
	20	19	17	15	15	14	13	12	13	14	14	15	16
	20	18	16	15	15	14	12	12	12	13	14	15	15
	20	20	15	15	15	14	12	12	13	13	15	15	16
	20	18	17	15	15	14	14	12	13	13	15	15	15
m. v.	.8	.6	.8	.5	.4	.6	.7	.3	.5	.2	.6	.4	.4
Second half.													
	17	15	16	13	13	12	12	13	13	14	14	17	20
	17	17	16	14	13	14	14	12	13	14	14	14	20
	18	16	15	14	13	13	11	12	12	13	13	15	17
	18	16	14	14	13	13	12	13	14	12	14	15	16
	16	16	16	13	13	14	12	13	13	14	14	16	16
	17	16	16	14	13	14	12	13	13	13	14	14	14
	17	16	15	15	13	13	11	12	14	13	15	17	17
	17	15	15	14	13	13	13	11	13	13	14	15	17
	19	15	16	13	12	14	12	13	14	13	14	15	16
	17	15	15	15	14	12	12	12	13	13	15	15	15
	18.9	17.5	16.2	14.7	13.9	13.4	12.4	12.3	13.1	13.1	14.1	15.0	16.0
m. v.	.6	.6	.6	.5	.2	.6	.6	.6	.5	.5	.4	.8	1.4

The curves—Instead of giving all the records in this form, the results may be represented more advantageously and clearly in graphic form. Figures 1 and 2 contain the records of the two observers from each of whom ten records were obtained. Fig-

ure 3 presents the composite curves. Each one of the light curves represents the averages of all the determinations made at

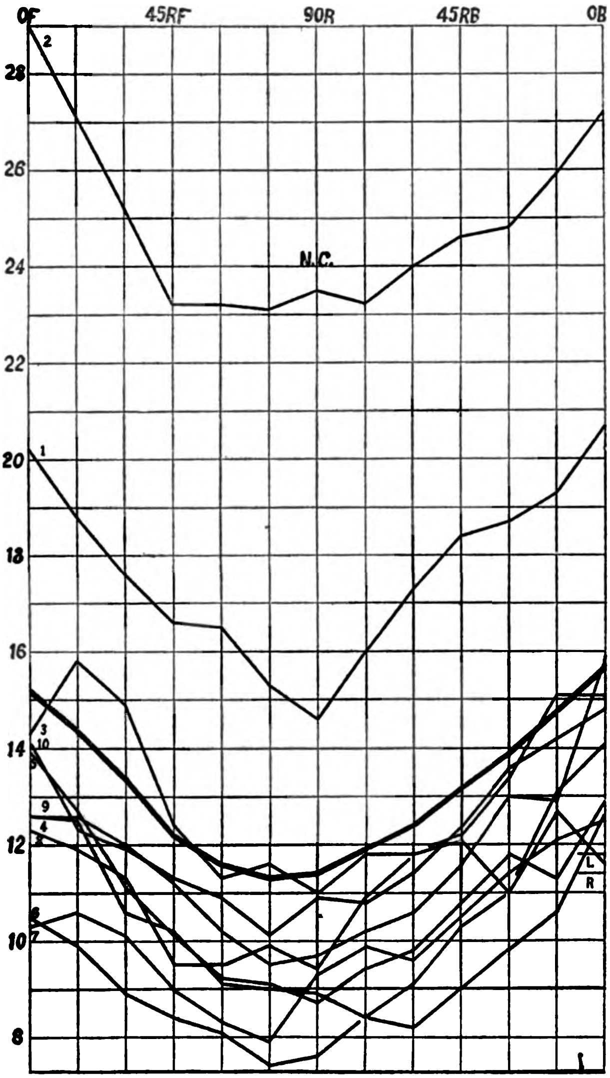


FIG. 1

each direction in one record. The heavy curves are the average curves. The numbers at the top are the designations for the

directions. The large numbers on the left are the steps on the audiometer scale, and the small numbers at the beginning of each curve indicate the order in which the records were made. The two bars at the right indicate the thresholds of the two ears obtained before the records were begun. The lower a point is in the curve the lower is the threshold; that is, the keener is the sensibility.[1]

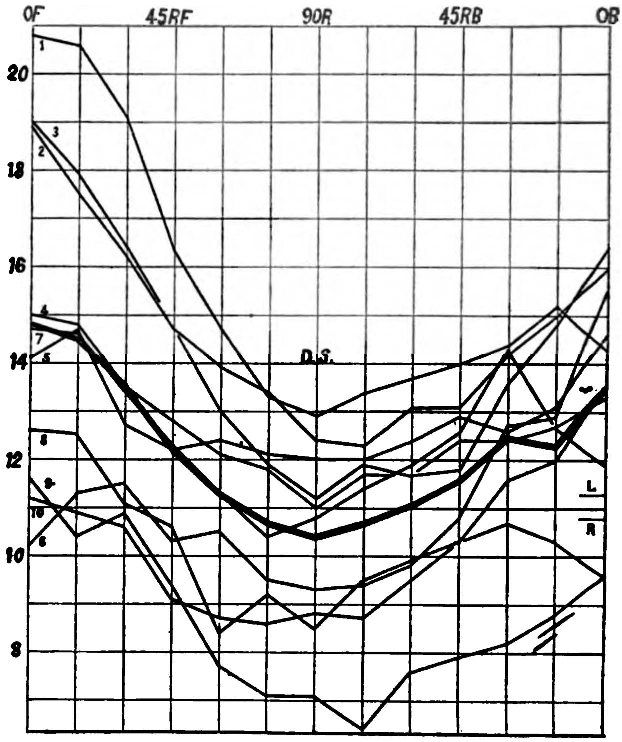


FIG 2

The individual records of the other observers are not presented in individual curves but in the statistical form of Table I. Each horizontal row of numbers is a record. Each num-

[1]The first two curves in Figure 1 are considerably higher than the others. This is due principally to the conservative standard of responding which the observer had adopted. She did not respond until she had "verified the hearing of the sound." In the latter records a less conservative standard was adopted.

ber is the average of the twenty measurements. The upper part of Figure 3 is a composite curve based upon this table.

TABLE I.

0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
C. E. S.												
15.6	14.4	13.9	11.8	11.2	10.4	10.0	11.1	11.2	11.9	15.3	15.5	16.5
15.1	14.6	12.7	12.3	11.2	10.5	10.1	11.0	11.5	12.2	13.8	14.5	16.5
E. A. J.												
20.5	18.5	14.9	13.6	11.7	10.5	10.2	10.8	10.8	12.1	13.8	15.3	15.3
16.1	14.7	14.0	13.1	11.4	10.2	10.1	11.3	11.3	11.2	14.2	14.7	15.3
N. B.												
13.2	13.1	12.2	11.6	10.9	10.1	9.7	10.3	11.0	12.6	13.6	13.8	12.7
21.7	21.5	20.9	18.7	17.6	15.9	15.7	16.8	16.8	18.6	19.6	19.2	20.2
20.1	20.0	19.5	17.5	17.0	15.6	14.5	15.8	16.9	18.4	18.9	18.2	17.9
E. G. Q.												
29.3	28.6	27.3	26.3	25.1	24.5	24.2	25.3	25.6	26.6	27.4	28.1	27.7
27.3	27.3	26.3	25.4	23.8	22.3	22.8	22.9	24.1	24.8	26.5	27.5	28.0
32.3	33.0	32.3	30.2	30.2	30.0	29.5	30.4	31.1	31.9	30.0	31.5	34.6
R. W. S.												
20.4	18.7	16.0	14.9	13.3	12.7	12.8	13.5	13.0	14.2	15.9	17.2	17.6
15.0	15.6	15.1	14.7	13.8	13.2	12.9	13.6	14.6	13.8	14.9	14.8	14.5
H. S. B.												
24.3	22.3	20.9	20.1	20.2	18.4	18.0	18.2	20.6	21.4	22.1	22.8	24.2
17.7	14.4	13.7	12.1	10.9	10.9	10.7	11.7	12.9	13.5	16.3	19.9	20.6
26.6	26.0	24.4	23.8	23.0	22.5	22.3	23.7	25.0	25.8	26.9	28.6	27.2
Averages.												
21.0	20.2	18.9	17.8	16.8	15.8	15.6	16.4	17.1	17.9	19.3	20.1	20.6

The mean variation for these records is not given because it is relatively small in all the measurements. The mean variation in the sample record above is representative.

Data in the curves—(a) The region of keenest sensibility is at the side, at 90°r. (b) Front and back are considerably less keen than the side. (c) There is a difference among the observers in regard to the acuity in front and in the back, which divides them into two distinct types: those who have greater keenness in *front* and those who have greater keenness in the *back*. (d) The remaining directions occupy intermediate positions so that

the curves are free from breaks and present a relatively smooth appearance. The curves approach the shape of a flattened U.

If we compare the curves of Figure 3 with the other curves we notice at once the general prevalence of these features. Although there is no absolute guarantee that the stimulus from the fork remained constant through the tests, there is every reason to believe that it did remain practically constant. We may, at any rate, safely assume that there were no serious changes

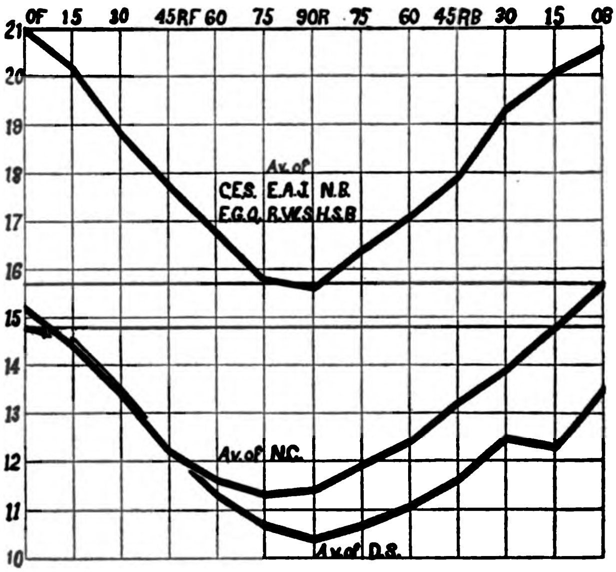


FIG. 3

for periods as long as the taking of a complete record. In order not to disturb the uniformity of the stimulus the accessories of the apparatus, such as the wiring, fork contact and the battery current (from three Edison-Lelande cells), were kept as uniform as possible.

Comparison of side with front and back—The keenness of the side as compared with front and back may be expressed in a ratio. In the average curve of N. C., Figure 3, the lowest, i. e., the keenest, point at the side is 11.3 and the average for front and back, 0°f and 0°b, is 15.5, giving the ratio 11.3 to 15.5 or

.73. The ratio of the average curve of D. S. is .72 and for the average curve of the other observers .75. It seems then that irrespective of the absolute thresholds, without regard to the relative keenness or obtuseness of hearing, the ratio is constant, being approximately 3 to 4. Whether the curve is higher up in the scale or lower down, its general form will be the same. See for example the individual records in Figures 1 and 2 and in Table I. This undoubtedly finds its explanation in the fact that the graduated scale of intensities on the audiometer is based on Weber's law, making the increments on all parts of the scale equally perceptible.

Comparison of threshold with "distance" tests—In Part I[1] a series of tests was made to determine the apparent distance of a sound of uniform intensity and distance in the same group of directions for which the threshold has been measured. It was there found that the same sound was estimated nearest at 90°r and farthest away in anterior and posterior directions. At 90°r it was estimated to be at a distance of 38.3 inches, while at 15°rf it was estimated to be 44.4 inches, and at 15°rb, 44.6 inches.[2] Thus the curve of the distance tests agrees with the threshold curves. The threshold is lowest at 90°r where the sound seemed nearest; and it is highest in the anterior and posterior directions where the sound seemed farthest away.

Introspections—The following introspective notes are quoted as they were written by the observers immediately after each test. They express characteristic observations and experiences of the observers during the experiments. Unessential parts and repetitions in successive notes are omitted.

D. S. 10.00 a. m., Oct. 25, '04.

Had my eyes closed. Have a cold in my head. Mind wandering was rather disturbing in the first half. Felt quite comfortable and was not particularly wearied at any time. Sometimes it seemed that I could hear the sound all the time and that I responded when it seemed to become louder. At the point 0°b the sounds seemed to be 15° or 20° to the left of 0°b. There was no tendency toward rhythmic response.

[1] See Part I, 20–23.

[2] 0°f and 0°b seem to be exceptions; they were estimated to be nearer than 15°rf and 15°rb.

D. S. 10.00 a.m., Oct. 27, '04.

Still have quite a severe cold in my head and occasional singing in my left ear. Noticed especially the rushing of blood in the head and the heart beating. Sometimes the sound seemed to come in pulsations corresponding to the heart beats but I could not determine whether they coincided. Could not notice any qualitative differences in the sound from different directions. Sometimes it seemed that the source of the sound was nearer at the side than in front or in the back. I do not think that my standard of certainty in responding changed noticeably during this test. My attention varied considerably. About half of the time it was mostly passive. Had my eyes closed because I feel that I can then pay attention better.

D. S. 10.00 a.m., Oct. 29, '04.

I anticipate that the thresholds in this record are lower than in the preceding ones because I seemed to attend better to the stimuli, and my cold is considerably better. At the side the sounds usually came in pulsations corresponding to the heart beats felt in the ear, and the distance of the receiver seemed less at the side than in front or in the back. It seems also that somehow it was easier to hear the sounds from the side, that is, I feel sure more quickly of hearing the sound there than at the back or in front.

D. S. 10.00 a.m., Nov. 1, '04.

Had eyes closed. Felt fresh and vigorous. Mind wandering was most disturbing at 75°rb, 30°rb, 15°rb and 0°b. At these positions there was a dull feeling in my ears. Sounds at the side seemed more piercing and usually came by pulsations. The standard of certainty was the same throughout and about the same as in previous tests. It seemed also that the process of perception and the feeling of certainty were simpler and easier at the side than either in front or in the back.

D. S. 10.00 a.m., Nov. 3, '04.

Felt fresh and bright. Had eyes closed. Sometimes the sounds seemed to come in by pulsations but I had no especial associations with them. The attention fluctuated more than in any preceding test. Often I was attending only passively to the stimulus. Frequently at the side and in front I seemed to hear the sound all the time and consequently it was difficult to stick to a standard. My visualization which is always present underwent several very noticeable changes. Beginning with 0°b in this test I visualized a line extending from the back of my head to the receiver. At 30°rb this changed and the line seemed to extend from the right ear to the receiver, until we came to 15°rf and there the line seemed to extend from the center of my forehead to the receiver.

D. S. 2.30 p. m., Jan. 28, '05.

I was in excellent condition for the test. Had eyes and mouth closed. There was a tendency toward mind wandering during the first fifteen minutes, covering the first three points. The rising of the sound to and above the thresh-

old reminded me of breathing that begins slowly and increases in rapidity and volume as the air is expelled from the lungs. The visualization had the same peculiarities as in the last record. There was a slight tendency toward rhythmic response.

D. S. 8.00 a.m., Feb. 1, '05.

Was not subject to hallucinations so much as before. Noticed very distinctly the changes in the process of visualization, mentioned before. I felt very sure of such changes and possibly they may have been a cause for the difference of threshold in different directions. I also noticed clearly a feeling of strain in my right ear when I listened 'mainly with that ear,' i. e., on the side, which I did not notice in the front or in the back. I also felt quite sure that subjectively the main difference between the side and front or back was that I felt certain more easily and quickly in the measurements on the side, as to whether I heard the sound or not; and possibly that may be one reason for the threshold being lower at the side than in front or in the back. I seemed to feel more at ease when the sound was at the side.

D. S. 8.00 a.m., Feb. 3, '05.

Was somewhat disturbed by hallucinatory sounds especially at the first few points in the beginning of the second half. Also noticed a decided feeling of strain and heaviness in both halves in the range of 45°rf to 45°rb. In the first half at 75°rf it seemed that the sound became a little higher in pitch and apparently remained there during the rest of the experiment. Sometimes it seemed that the perception of the sound was not so much the observation of a sound coming above the threshold as a gradual discrimination of it, as it became louder, from the subjective sounds which were conspicuous on account of the quietness of the room.

D. S. 8.15 a.m., Feb. 8, '05.

Began the test with a special determination to make as uniform a record as possible, that is, to make the mean variation as small as possible and have the standard of certainty the same throughout as well as I could. Again noticed very distinctly the change in visualization. Also observed that as the sound came into consciousness I heard the overtones before I heard the fundamental. First I heard the fifth, then the third, and finally the fundamental. Sounds scarcely ever came above the threshold in any other way in today's record, especially in front and on the side. Did not notice it so frequently when the sound was in the back.

N. C. 3.30 p.m., Nov. 8, '04.

The quality of the sound seemed constant, but at 0°f and 0°b the sound was hardest to hear. The places where it was hardest to hold the attention were about 45°rf and 45°rb. From 45°rb to 0°b the distance seemed twice as great as in the other directions. Fatigue was not noticed until the last quarter of the experiment.

N. C. 3.30 p.m., Nov. 9, '04.

Several images (associations) were noticed; at one time the sound seemed like the singing of a mosquito, and again like the buzzing of a track when the cars or train is a long distance away.

C. E. S. 8.00 p.m., Oct. 22, '04.

Kept mouth open because of cold. Felt more wearied just before the middle than at any other time. The first mind wandering was in the first round at 45°rb. Kept eyes closed.

The sound had entirely different qualities in different directions. In the back I do not get certain overtones. There is a point about 30°rb where there is difficulty in choosing whether to listen to the overtones or to the fundamental. The sound comes in by pulsations, about three a second, which may be heard as long as the sound lasts. The certainty is not great. I was more certain in the front quadrant than in the rear; that is, the sound seemed more distinct, rather than a change in my standard of certainty.

C. E. S. 9.00 p.m., Nov. 1, '04.

Kept mouth closed all the time. Very few trials had to be repeated. In the first half I seemed to judge by fainter standards than last time. In the last half of the latter part of the record, the standard seemed a good deal clearer than before but it was impossible for me to hear it until it had this clearness. The sound came in as beats. These beats had association with distant sounds, e. g., it was difficult for me to avoid thinking of the sound as coming from a rooster, a dog, distant singing or speaking. There were periods when I judged by these associations instead of thinking of the sound as a meaningless threshold sound. The difference in quality was not so marked this time as last. Both active and passive attention are present—the former only for a short period. Can it be that the beats are due to the 'vibration' of active effort? I located the sound in one ear all the time. The perception of direction was much less certain than for strong sounds. The sound was of higher pitch than the fundamental. Did not determine what it was—perhaps it was the fifth or octave.

H. S. B. 9.00 a.m., Nov. 8, '04.

In the experiment I heard overtones rather than the fundamental tone from 60°rf to 60°rb. In many cases I heard only a sort of fluttering noise which did not seem to have tone, but the presence of which could be distinctly detected. In nearly all cases the beginning of the sound as it came above the threshold was indefinite; but cessation of the sound was much more clearly distinguished. This gave a feeling of satisfaction in the reater certainty that the sound had really been heard and was not an illusion.

Most of the sounds were localized somewhere within my skull, some being rather high and to the front, and others being in the lower back part. About 45°rf and 45°rb there was a less degree of certainty as to when the sound began and stopped than was true of other locations.

In order to procure more specific information on some points the following list of questions was submitted to the observers after all the tests had been completed:

1. Did the sound seem different from different directions? Did you notice overtones?
2. Did you have any associations with the sound? Did the sound come by pulsations?
3. Did the receiver or source of sound seem to change in distance for different directions?
4. Was it easier to perceive the sound in some directions than in others? Which and Why?
5. Did your standard of certainty in perceiving the sound change?
6. Was there a tendency toward hallucinations and rhythmic response?
7. Did you visualize the position and direction of the sound? Was the visualization different for different directions?
8. Did you have difficulty in attending to the stimuli?
9. Did you notice any motor sensations?
10. Eyes and mouth closed or open?

The more significant observations brought out by the introspective notes and the replies to the questions may be summarized as follows:

In the beginning of the tests there was more or less uncertainty as to just when to feel sure that the sound was heard. But as the experiments progressed a fairly constant degree of confidence was adopted by each observer.

In several instances the sound was heard in pulsations, about three per second in one case, and apparently correlated with the heart beats in another.

The attention fluctuated very noticeably in most cases, which is indicated not only by these introspections but also by the actual fluctuation of sensitivity (see Figure 5). The introspections also seem to agree that it was more difficult to hold the attention during the measurements in the lateral directions than in the other directions. Some claimed to be able to pay attention better when the eyes were closed and others when the eyes were open.

With some observers visualization of the source of sound and associations with the sound were very prominent and apparently of significance to them in the process of perception. Several striking contrasts and changes accompanied the measure-

ments in the different directions. The lateral directions were characterized by entirely different visualization processes than the anterior or posterior directions.

Interpretation of the curves—The three facts which demand consideration are, that the threshold for front and back is approximately the same, that there are two types of persons in respect to the relative keenness in front and in the back, and that the threshold for the side is decidedly keener than for either front or back.

The first fact plainly shows the error of the prevailing belief and statement sometimes made that we hear sounds from the front better than from the back. If we notice the conditions present in these two directions, front and back, and the similarity in their location, we are prone to ask, why should the two be very different? Indeed the agreement in the keenness in these two directions, might have been predicted on theoretical grounds, inasmuch as these two directions are located symmetrically with respect to the ears, and the ability to localize sounds in these two regions is the same.[1] The belief that hearing from the front is finer than from behind undoubtedly rests partly upon the shape of the concha and partly upon the observation that we tend to face the source of sound when we wish to hear well. An observation made by v. Kries[2] undoubtedly finds its explanation in the above results, "———wir konnten z. B. nicht finden, dass etwa der schwächere Klang mit Vorliebe nach hinten, der stärkere nach vorn verlegt worden wäre."

In regard to the keener sensibility at the side let us first consider the introspective remarks on the characteristics, conditions, and processes of perceiving the sound in the lateral directions. An observation frequently recorded by the majority of the observers is that the sound from the lateral directions seemed nearer than from the anterior or posterior directions. But this is simply a naïve statement of what is objectively shown by the experiments, namely that since the threshold is lower at the

[1] See localization charts in Part I.

[2] Ueber das Erkennen der Schallrichtung. Zeitsch. f. Psych. u. Physiol. d. Sinn., I., 1890, 246.

side a sound is comparatively higher above the threshold, that is, relatively stronger and hence nearer.

A still more significant fact is the repeated statement in the introspective notes that it is 'easier to perceive the sound' when it comes from a lateral direction. The same observation was stated in various ways. 'I feel more at ease,' or 'more confident' when the sound is on the side. It is 'more piercing' on the side. 'Hard to pay attention,' or 'was not so sure in response when the sound was behind me.' 'Hardest to hear in front and back.'

Then there were striking changes in the forms of visualization in symmetrical places in the front and rear quadrants. For example, see above the note of D. S., Nov. 3. The shifting of the visualization process and the changes in the facility of the attention seem to have accompanied each other. The question arises, were there similar changes in the process of perception in passing from anterior or posterior directions to lateral directions? One observer mentioned (see above H. S. B., Nov. 8), that the overtones in the directions between 60°rf and 60°rb were much more prominent. The fact that the source of sound at the side is more favorably located with reference to the ear on that side no doubt accounts for the lower lateral threshold and for the qualitative changes mentioned by the observers. Do we not actually take advantage unconsciously of the keener sensibility on the side? In an audience one may frequently observe people trying to hear the speaker better by turning the side of the head toward him.

The individual difference among the observers in regard to the acuity for front and back divides them into two distinct types: (a) those who have greater keenness in the front and (b) those who have greater keenness in the back. The discovery of this difference in the first few tests led to the two extended series of records shown in Figures 1 and 2. The object was to determine whether this distinction would be maintained permanently or whether it was only an individual deviation which would be counterbalanced by additional tests. But the results show that the two types were clearly maintained. The difference for front and back for these two types is .5 for

N. C., Figure 1, in favor of front, and 1.3 for D. S., Figure 2, in favor of back. This difference is even more conspicuous in the monaural records of the same observers, Figure 4, being 2.7 for N. C. in favor of front and 1.3 for D. S. in favor of back. The amount of difference is too great to be accidental, as in each case except the first it is more than one unit of measure on the audiometer.[1]

What accounts for these two types? One factor that might be suggested is undoubtedly the differences of the anatomical structures of the ears, especially the pinnae. A slight difference in the course of the meatus and in the adjoining structure may also possibly render the perception of sound easier from the rear in one individual or easier from the front in another individual.

Another reason for the keenness ofdetecting sounds from the rear may be sought in the phylogenetic development of the race. The ear rather than the eye has been the means of detecting sources of danger in the rear, and consequently the auditory habits have adjusted themselves to serve this purpose.

Only one series of directions, the horizontal plane through the aural axis, was tested in this series, but the results may safely be generalized and applied to the vertical planes as well, on the ground that there is uniformity in the localization records for the horizontal and vertical planes and uniformity in general conditions for these two sets of planes. If one of the composite curves, Figure 3, were revolved on the point 90°r as the center in such a way that its axis would coincide with the aural axis, it would generate a saucer-like surface which would probably represent the keenness of sensitivity for all possible directions of the right hemisphere and analogously also for the left hemisphere.

Monaural Threshold.

In order to measure the monaural threshold and to compare it with the binaural, six monaural records were obtained from N. C. and D. S. The conditions and method of measurement

[1] One unit can be perceived as an increment with a fair degree of certainty.

were exactly the same as in the preceding experiments, excepting that the left ear was heavily bandaged.

Table II gives the individual records. In Figure 4 the average monaural and binaural curves of the two observers are presented together for direct comparison. The binaural curves are taken from Figure 3.

Table II.

N. C.

0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
13.8	12.5	11.9	12.0	10.8	10.3	10.0	10.8	11.7	12.2	14.0	15.8	16.9
13.2	11.6	11.3	10.7	10.3	9.2	9.2	9.0	9.8	10.8	11.7	12.0	15.0
12.2	11.9	10.6	10.5	9.7	8.7	8.6	9.3	9.5	11.0	12.7	14.4	15.6
13.1	12.0	11.3	11.1	10.3	9.4	9.3	9.7	10.3	11.3	12.8	14.1	15.8

D. S.

16.5	14.9	13.7	13.2	11.4	10.8	10.7	10.8	11.1	11.5	12.5	13.2	14.6
16.8	16.8	15.3	13.9	12.5	11.1	11.3	11.7	12.4	12.1	12.5	13.8	14.8
15.5	15.7	14.5	12.3	11.1	10.8	11.6	11.9	12.4	12.8	13.5	14.2	15.5
16.3	15.8	14.5	13.1	11.7	10.9	11.2	11.5	12.0	12.1	12.8	13.7	15.0

The mean variation for these records is about the same as for the binaural measurements, about .6.

In general the results present no new features: front and back are approximately on the same level; both observers maintain their respective types, N. C. with lower threshold in front and D. S. in back; the side is considerably keener than either front or back.

The monaural threshold in the lateral directions is practically the same as the binaural, but in the anterior and posterior directions the monaural threshold is a little higher. The ratios of side to front and back in the monaural tests are .64 (N. C.) and .69 (D. S.) while in the binaural tests they are .73 (N. C.) and .72 (D. S.). Evidently the exclusion of the left ear does not affect the lateral thresholds on the right side, but the anterior and posterior are slightly higher.

It is evident then from the close agreement of the monaural with the binaural thresholds that the acuity of hearing is not dependent upon any coöperation of the two ears so far as the hemisphere on the side of the active ear is concerned. On the side of the hearing ear we hear as well with one ear as with two. But in respect to discriminative processes, such as are involved in the auditory perception of direction, the combined action of two ears is decidedly better than monaural perception. This will be considered later in greater detail.

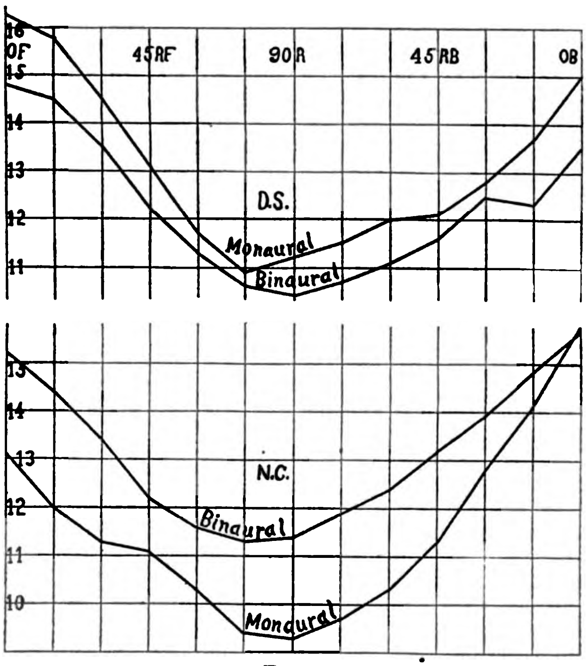


FIG. 4

The Bearing of the Threshold Experiments upon the Localization of Sound.

The localization of sound has been shown to be much less accurate on the side than in front and in the back. The threshold of hearing, on the other hand, is considerably lower, that is, keener on the side than in front and in the back. The curves of localization and of the threshold measurements take

directly opposite courses. Hence, acuity and localization do not run parallel and are not direct functions of each other, nor are the two curves reciprocals of each other. The ratio of the thresholds of the side to front and back is, as shown above, 3 to 4, while the same ratio for the localization is about 4 to 1.

Localization depends upon discrimination, while hearing ability or acuity is an expression of sensitivity. Localization is primarily a binaural process while hearing ability is mainly a monaural matter. Localization is most accurate in anterior and posterior directions where the coöperation of the two ears is at a maximum; it is least accurate in the lateral directions where the coöperation is at its minimum. If one ear be excluded the localization will be greatly impaired in those directions in which the coördinating activity is most important,[1] but on the other hand, the hearing ability will remain practically unaffected. Localization involves processes of discrimination and is primarily binaural, just as the perception of visual space and volume is dependent largely upon binocular vision, whereas the sensibility to light is probably as acute in one eye as in two. Of course, monaural localization is possible just as monocular space perception is possible, but it is not as accurate and reliable.

Sensibility and discrimination do not necessarily depend upon each other nor vary together. A person with a low threshold will not necessarily localize more accurately than one with a high threshold.

The significance, then, of the present threshold measurements for localization is this: They demonstrate precisely what had been merely a supposition, namely, that a sound on the side in the region of the aural axis does seem stronger and nearer than in front or in the back, and of this difference we have obtained a quantitative measurement. A sound seems stronger when near the aural axis than when farther away and these variations in intensity are potent factors in rendering our perception of direction accurate.[2]

[1] See the last series in this report on monaural localization.

[2] These results corroborate the discussion in Part I, 17 ff. Further, the equal sensitivity to sound in front and in the back contradicts the popular assumption that we tend to place weak sounds toward the rear and loud sounds toward the front.

General Observations.

There are two main individual differences in auditory sensibility. First, in respect to the relative acuity in front and in the back, individuals are divided into two classes. Second, the threshold is much higher for some than for others. Compare for example the records of C. E. S. and E. G. Q. in Table I. The threshold of the latter is about twenty units higher. This may be due to differences in mental attitude during the tests, in the standard of certainty in responding, or in the anatomical structure of the sense organs.

The auditory acuity is greater on some days than on others; compare, for example, records 6 and 10 in Figure 1. These daily fluctuations find their explanation in the variation of subjective conditions and perhaps also to a slight extent in the unavoidable objective variations.

The records on successive days[1] show some improvement. There is a tendency for successive curves to be progressively lower in the scale. This improvement is probably not in the sensibility of the ears but rather in increased familiarity with the sound and consequently increased power to direct and control the attention in the experiments, and possibly also in the adoption of a fairly constant standard of certainty. The uncertainty as to whether the sound was heard or not, disappeared after the first record. The improvement is thus due to practice, and to familiarity with the situation of the experiment, rather than to any increase in the actual sensitivity of the sense organ.

The recent demonstrations of fluctuations and periods in mental acuity and application[2] show that these factors enter into the results of all forms of psychological experiments and particularly into continuous work such as was required in these threshold measurements. Each record represents practically continuous activity for approximately forty-five minutes. The

[1] The order of the records is indicated by the small numbers at the left end of the curves.

[2] Kraepelin, "Die Arbeitscurve." Phil. Studien, XIX, 1902, 459-507. Seashore and Kent, "Periodicity and Progressive Change in Continuous Mental Work." Univ. of Iowa Studies in Psych., IV, 1905, 47-101.

only breaks occurred when the observer had to turn from one direction to the next. Although no special effort was made to follow any set time, the stimuli and the reactions followed quite uniformly at intervals of from 5 to 8 seconds.

To show the fluctuations and to bring out the types of periods discovered by Seashore and Kent the following two curves, Figure 5, are presented from the records of N. C. and D. S. Each curve represents one complete record. The points in the zigzags are individual reactions. The horizontal bars represent the averages of groups of the ten determinations which were made at a time in a given direction. The breaks in the curve between the groups of tens represent the change in position on the part of the observer from one direction to the next. Each curve is composed of two parts, being the two parts of the double fatigue order from 0°f to 0°b and then from 0°b to 0°f. The continuous line in each curve is the composite of the ten records obtained from the two respective observers, and is to be regarded as the basis upon which the zigzag curve is drawn, the latter being one of the ten records in detail. The composite curve is used as the basis because all the detailed fluctuations are obliterated in it and it thus affords the only standard by which to detect the individual deflections of the separate records. It must be borne in mind that the wave-like form of the average lines is the typical variation in the threshold for the different directions as pointed out above, and not the fluctuation in mental work. Regarding the continuous lines as the base lines we notice that each record shows two complete waves of fluctuation. For example, the upper curve begins above the base line, gradually falls below it, rises above again at 0°b, then falls below reaching its lowest point at 45°rf when it rises to the level of the base line at 0°b. These large waves correspond to the small hour waves of Seashore and Kent.[1] The zigzags of the separate reactions probably coincide with the crests of the "second-waves" six to eight seconds long, which was approximately the period of one reaction. Then in the groups of tens there are frequently smaller groups of two to seven reactions

[1] Op. cit. p. 55.

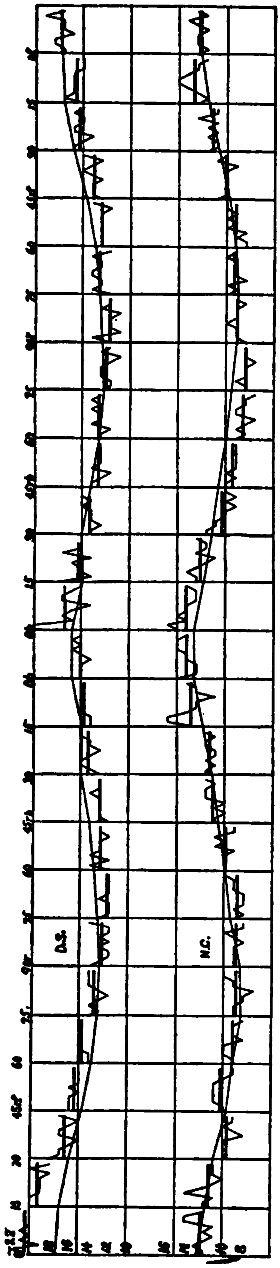


FIG. 5

which are either all above or below the horizontal bars. These correspond to the 'minutes waves.'

The periods of drowsiness and difficulty in paying attention which were experienced by the observers during the experiments probably coincide with the larger fluctuations. From the point of view of the present tests these fluctuations in mental work are disturbances and account for the deviations of the individual curves from the averages. But their effect is not serious because it is neutralized in the composite curves.

SERIES II.

Discrimination of Intensity and Pitch in Different Directions.

The differences in threshold in different directions as found in the last series of experiments suggested the problem as to whether the keenness of discrimination between intensities or pitches or other qualities of sound would also vary with different directions. The aim of this series of experiments was to determine whether the discrimination varies in some regular manner for the series of directions in which the hearing ability measurements had been made.

Discrimination for Intensities.

The apparatus, accessories, source of sound, location and distribution of the apparatus, were exactly the same as in the threshold experiments. The only difference was in the method.

Before the regular tests were begun the threshold of hearing was found for each observer and then a sound ten units stronger (on the audiometer scale) was chosen as the standard intensity. This sound was strong enough to be easily heard.

In making the measurements the experimenter gave the standard sound for one second and then after an intermission of one second, sounded for one second either the standard or the sound one unit stronger. The problem for the observer was to deter-

mine whether the second sound was the same or a stronger. If he judged it to be the same he responded with one tap on the strap key; if stronger, with two taps. The increment of one unit was found to be large enough to be perceived as an increment and yet not too large to make the observer absolutely certain. Ordinarily about 75% of the judgments would be correct.

Twenty determinations were made in one sitting, in the double fatigue order, for each one of the thirteen directions on the right side. This constituted one record.

The tests were made on three observers (men), C. E. S., E. A. R. and D. S. For the first observer, the standard intensity on the audiometer scale was thirty, for the other two, twenty-five. Two of the observers made three records each, and the third made two records. Thus 160 determinations were made for each direction, or a total of 2,080.

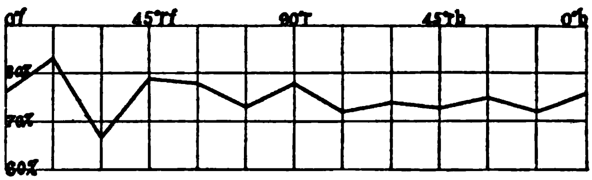


Fig. 6

The results of these measurements are represented in the curve of Figure 6, which is the composite of all the records. The points in the curve were determined by calculating the percentage of correct responses.

It is evident from a glance at the curve that the discrimination between intensities is the same in all directions, each showing about 75% correct judgments. If a still larger number of records had been obtained the curve would probably have become almost a straight line. The results, although largely negative, corroborate the suggestion made above that keen sensitivity is not necessarily accompanied by accurate discrimination. It also demonstrates that the greater accuracy in localization in front and in the back than at the side, is not due to greater accuracy in discriminative ability in those directions.

Discrimination between Pitches.

The apparatus employed for the purpose of determining the discriminative ability between pitches was a set of tuning forks customarily used in the Iowa laboratory. It is a set of eleven forks of uniform size and shape—11.5 cm. long—tuned to produce successive increments above the pitch of international A, 435 v.d. as follows: ½, 1, 2, 3, 5, 8, 12, 17, 23, and 30 v.d.

The sound of the forks had to be augmented by a resonator because the source of stimulation had to be at a distance of one meter from the observer's head. An upright glass tube served as resonator. The experiments were made in the quiet room of the laboratory.

In making the tests the standard fork—435 v.d.—and one of the differential forks were sounded in rapid succession by striking them uniformly and holding them over the resonator. The observer was allowed a choice of only two answers, namely whether the second tone was higher or lower than the first. The approximate discriminative sensibility of each observer was found by a few preliminary trials. Ten trials were made at one time in one direction. If less than 70% were correct a larger increment was taken, if more than 80% were correct the increment was decreased. Twenty trials for each of the thirteen directions were made in one sitting in the double fatigue order.

The increment required for 75% correct judgments was calculated from Fullerton and Cattell's table.[1] The narrow limits of 70% and 80% were chosen as the data for the calculation in order to make the calculated increment reliable. An increment calculated on the basis of a record in which 60% or 90% are correct would not be empirically valid, especially when the increments are so small as in the present tests, being within .5 and 2. v.d.

Twelve records were obtained—from C. E. S. one, from E. A. J. six, and from D. S. five. These gave 240 trials for each direction or a total of 3,120.

[1] "On the Perception of Small Differences," Univ. Penn. Phil. Series, No. 2.

The variation of the observers from one another was within a narrow range (.8 to 2.1 v.d.) so that it is not necessary to present the records individually. They are summarized in the composite curve in Figure 7. The abscissae represent the directions and the ordinates represent the amount of difference in pitch, measured by vibrations.

The interesting feature of the curve is that the discrimination is considerably poorer on the side than either in front or in the back, the front is slightly better than the back. Although the difference in discrimination between $0°r$ and $0°f$ is only .5 of a vibration, it is relatively large, amounting to over $40\,\%$. All the

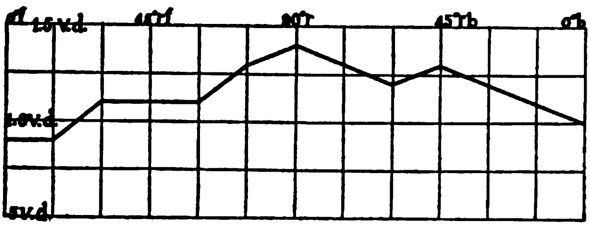


Fig. 7.

records agree in this respect. As an explanation of the poorer discrimination at the side it might be suggested that the ear probably catches more overtones when the source of sound is on the side. This may be confusing and consequently cause poorer discrimination.

The localization of sound is more accurate in front and in the back than on the side. Likewise the discrimination between the qualitative characteristics of tones, according to the pitch tests, seems to be better in front and in the back than on the side. It appears reasonable to infer that the greater accuracy of localization in front and in the back may be due in part to the greater accuracy in discriminating between the qualitative characteristics of sounds in those directions.

SERIES III.

Quality[1] of Sound in Localization.

It became evident from the experiments on localization described in Part I that the character of the sound is of considerable importance as a datum for discerning direction. As demonstrated by some special tests[2] the sound seemed clearer, richer, and fuller in some directions than in others, and these qualitative differences are constantly made use of unconsciously to differentiate the directions of sound.

The problem then is, can a sound whose complexity and qualitative characteristics are reduced to a minimum be localized as accurately as a richer and more complex sound? To what extent does localization depend upon these characteristics?

It is quite evident that this problem can be approached by choosing a variety of stimuli differing as widely as possible in their nature and complexity, and comparing the accuracy with which they can be localized. The following stimuli were selected.

a. The singing flame. This was produced in the usual manner. Ordinary illuminating gas, containing about 60% hydrogen, was used. The resonator tube (glass) was 25.5 cm. long and 1 cm. in diameter. The height of the flame was 8 mm., and the pitch of the sound about 730 v.d.

It was necessary in the experiments to interrupt the sound when moving from one direction to another. This was accomplished by shutting off the supply of gas for an interval long enough to stop the tone, after which the flame would again rise to its usual height and produce the sound. Although the singing flame is not strictly pure it was considerably purer than any one of the other sounds used.

b. The Galton whistle. Three pitches were used, 10,000, 20,000 and 30,000 vibrations. The air current was supplied by constant pressure tanks.

[1] The term quality is here used as physicists use it, in the sense of timbre.
[2] Part I, 18 ff.

c. The voice. The word 'now' was pronounced with the intensity and clearness of ordinary speech.

d. An electric hammer. A small wooden hammer, 2.5 cm. long and 2 cm. in diameter, was struck against a block of wood 5.2 cm. long and 2.7 cm. in diameter. The hammer and block were mounted on an ordinary electric bell, the block was put in place of the gong and the hammer fastened to the armature. The automatic make and break was removed so that the strokes of the hammer could be controlled as the experiment required. The use of magnets in the circuit of a constant current by which the hammer was struck made it possible to produce strokes of uniform amplitude.

e. A clapper. A small board, 12.5 cm. x 6 cm. x 5 cm., and another piece of wood, 15 cm. x 2 cm. x 1.5 cm., were fastened together at one end by a hinge and a spring. By opening the clapper and releasing one wing, it would strike against the other wing producing a clashing noise. The clapper was manipulated by a string.

f. A whiff of air. The air supplied by pressure tanks was conducted through a rubber tube terminating in a small glass jet. The whiff was produced by opening and closing the rubber tube which furnished the air current.

These various devices for producing stimuli were attached to the arms of the sound perimeter. In case of the voice the experimenter stood in such a position that his mouth took the place of the mechanical devices. The method followed in the localization experiments of Part I was also employed here and the results were treated in the same way.

Three representative directions were chosen, 0°f, 45°rf, and 90°r, in the horizontal plane through the aural axis. It was not necessary to test more than these three directions for the purpose of comparing the different stimuli, because they represent the typical extremes in localization, and as demonstrated by the earlier experiments the curve of the right front quadrant is symmetrical with the curve of the right back quadrant.

The results are summarized in Tables III–V. The observers and the number of trials are mentioned in connection with each table.

TABLE III.

The Singing Flame.

Observers.	0°f	45°rf	90°r
N. C.	10.6	37.4	34.5
N. B.	6.4	15.8	29.8
H. S. B.	9.5	30.8	42.7
E. A. J.	14.9	30.0	*
Averages	10.3	28.5	35.7

* The discrimination at this point seemed to be too crude to make a satisfactory measurement.

TABLE IV.

The Galton Whistle.

Observers.	Pitch	0°f	45°rf	90°r
C. E. S.	10,000	5.4	*	46.1
	20,000	3.1		29.0
	30,000	6.2		54.6
D. S.	10,000	2.9	9.2	43.5
	20,000	2.5	5.8	34.8
	30,000	2.6	5.1	25.0
R. W. S.	10,000	2.1	12.5	28.5
	20,000	1.7	7.5	18.7
	30,000	1.7	8.0	23.1
A. K.	10,000	6.2	6.5	50.0
	20,000	8.8	13.1	35.9
	30,000	5.0	12.2	40.8
Averages	10,000	4.1	9.4	42.0
	20,000	4.0	8.8	29.8
	30,000	3.4	8.4	35.9

* No tests were made.

TABLE V.

Noises and the Voice.

	0°f				45°rf				90°r			
Observers.	Hammer	Clapper	Whiff	Voice	Hammer	Clapper	Whiff	Voice	Hammer	Clapper	Whiff	Voice
C. E. S.	1.8	1.5	2.6	1.5	4.4	3.4	7.2	2.6	10.6	37.7	25.6	9.5
D. S.	1.6	1.5	1.5	1.5	4.0	2.2	3.1	2.2	26.3	14.4	7.3	3.6
A. K.	2.4	1.5	1.6	1.5	3.6	3.1	3.6	2.4	10.5	12.8	11.6	9.2
C. L. V.	2.6	2.2	2.4	1.5	4.4	2.5	8.2	2.2	29.0	29.0	25.6	11.6
Averages	2.1	1.7	2.0	1.5	4.1	2.8	5.5	2.3	19.1	23.5	17.5	8.5

With each stimulus 50 determinations were obtained from each observer for each direction, total 2,400.

The figures in the table represent in degrees the just perceptible difference between directions. One hundred determinations were made with each observer for each direction, in all 1200.

In order to compare more directly the accuracy of localization of the different stimuli the results are presented graphically in the curves of Figure 8. The radii represent the directions and the arcs represent degrees of just perceptible difference between directions. The curve shows only the results obtained with the 30,000 pitch for the Galton whistle.

The main results may be summarized as follows: The singing flame, which is an approximately pure tone, is localized very poorly. The high tones of the Galton whistle which are

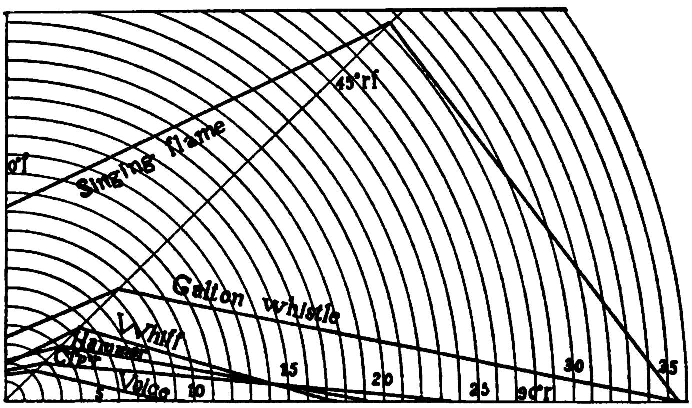


FIG. 8

quite free from overtones are also localized rather inaccurately. The noises are localized more accurately and the voice most accurately. The voice is probably not more complex than some of the noises but its overtones are more regular and continuous and within the customary range of hearing. In general, then the richer and more complex a sound the more accurately can it be localized. The complex sounds are the most frequent sounds in our experience, and the two factors, frequency and complexity, have coöperated in our learning to localize them

most accurately. These experiments corroborate the results obtained by Professor Angell in so far as a comparison can be made.[1]

The problem of quality was approached in still another way. An effort was made to obtain a tone that might be purer than the singing flame. So Helmholtz's method of using a fork and a resonator was adopted.[2] A fork of 250 v.d. and a Helmholtz resonator were mounted on a convenient wooden frame, which was attached to one of the arms of the sound perimeter. All points of contact were supplied with soft rubber in order to avoid any resonance from the frame. To eliminate the buzz caused by the interrupting contact, the current was interrupted by another fork of the same vibration frequency, located in a distant room. A double circuit was arranged so that the driving fork would be running constantly and yet at any moment the current could also be sent through the magnets of the stimulus fork for any desirable length of time. The tone thus produced was a close approximation to a pure tone, at least none of the other resonators in the Helmholtz series brought out any tones.

For the purpose of comparing the localization of this tone with a complex tone the sound from an ordinary telephone receiver was used. The current for this was interrupted by a 100 v.d. fork. The rest of the apparatus consisted of the sound perimeter by means of which the stimuli were controlled.

The method of experimentation may be described as a type of the method of production. Twenty-four directions, 15° apart, in the horizontal plane through the aural axis were used. The observers were told before the experiments that the stimuli would come from any of these points. The stimulus was given from one of the points and the observer then answered by means of the adopted nomenclature with which each observer had been made familiar before the experiments.[3] The experiments with the fork and the telephone were made simultaneously, that is,

[1] Angell, "A Preliminary Study of the Significance of Partial Tones in the Localization of Sound." Psych. Rev., X, 1900, 1–14.

[2] Helmholtz, "Sensations of Tone." Trans. by Ellis, third ed. 120, 339.

[3] See Figure 2 in Part I.

an equal number of determinations were made with both stimuli in each sitting. This was done in order to make the effect of practice and other conditions uniform for both. Thirty-six determinations were obtained for each of the twenty-four directions from six observers, four men and two women, in all 864 trials.

The results are given in Table VI.

TABLE VI.

Telephone.

Observers.	Number correct	Misplaced 15°	Confusions	Right-left misplaced	Others
D. S.	98	43	2		1
F. O. S.	74	56	5		9
R. W. S.	99	41	2		2
F. V.	78	47	8	1	10
E. L.	69	65	1	1	8
C. P.	97	43	1		3
Totals	515	295	19	2	33
Percent	59.6	34.1	2.2	.2	3.9

Tuning Fork.

Observers.	Number correct	Misplaced 15°	Confusions	Right-left misplaced	Others
D. S.	22	31	27	9	55
F. O. S	12	39	26	14	53
R. W. S.	19	33	22	13	57
F. V.	25	30	16	12	61
E. L.	33	41	13	13	44
C. P.	70	50		6	18
Totals	181	224	104	67	288
Percent	20.9	25.9	12.0	7.8	33.3

The significance of these figures is clear. They serve to emphasize the importance of quality and complexity of sound as data in the perception of direction. The complex sounds of the telephone are localized correctly 59.6% times as compared with 20.9% for the fork. But the difference is even greater when we compare the other columns of the table. More telephone sounds are localized approximately correctly, i. e., misplaced only 15°, so that if we add to correct localizations those

approximately correct we get for the telephone 93.7% while for the fork only 46.8%.

The column headed 'confusions' refers to those sounds which were located in a symmetrical position, or within 15° of the symmetrical position, in the quadrant on the same side. For example, if a sound at 60°rf was located at 60°rb, or within 15° of that, it is counted as a confusion.[1] These confusions are much more frequent with the pure tone than with the complex tone, the telephone 2.2% and fork 12.0%. The column of right-left misplacements refers to confusions, not necessarily symmetrical, of the right and left sides, and these also occurred oftener with the pure tone, 7.8%, than with the complex tone, .2%. The last column contains all those which are not accounted for under the preceding headings. These experiments, therefore, fully corroborate the conclusions of the preceding experiments, that a complex sound is much more accurately localized than a relatively pure tone.

It has been suggested that a pure tone can not be localized. While this cannot be positively contradicted on the basis of the preceding experiments, it nevertheless seems quite probable that an absolutely pure tone can be localized with some degree of accuracy. Even if the qualitative data in the perception of direction were completely eliminated in the case of a pure tone, the binaural ratio of intensities would still remain, by means of which some clue to direction would be indicated.

SERIES IV.

Misplacements in Localization.

Closer scrutiny of the errors and misplacements of the telephone sound reveals interesting tendencies to misplacement toward certain directions. The localizations of the telephone sound are therefore presented in detail in Table VII, in which the objective positions of the stimuli, the number of correct localizations, and the number misplaced (the coefficients) to

[1] Cf. Pierce, "Studies in Space Perception," p. 70.

the designated positions (in parentheses) are given. The four cardinal points, 0°f, 90°r, 0°b, and 90°l, are not included in the summary for reasons given below. The localizations of the telephone sound, rather than of the fork, were used because they are more accurate and bring out the misplacements more consistently and more rigidly.

TABLE VII.

Standard directions	Number correct	Misplacements.					
0°f	32	3(15°rf)	1(15°lf)				
15°rf	17	13(30°rf)	(0°f)	1(0°b)	3(45°rf)		
30°rf	21	12(45°rf)	1(15°rf)	2(60°rf)			55 backward
45°rf	26	5(60°rf)	3(30°rf)	2(75°rf)			15 forward
60°rf	23	10(75°rf)	2(45°rf)	1(90°r)			3 confusion
75°rf	19	7(90°r)	7(60°rf)	2(75°rb)	1(60°rb)		
90°r	19	6(75°rf)	6(75°rb)	4(60°rf)	1(45°rf)		
75°rb	12	15(90°r)	3(60°rb)	3(75°rf)	1(45°rf)	2(45°rb)	
60°rb	14	9(75°rb)	12(45°rb)	1(90°r)			45 forward
45°rb	22	5(60°rb)	6(30°rb)	1(75°rb)	1(0°b)	1(15°rb)	39 backward
30°rb	21	8(45°rb)	5(15°rb)	1(75°rb)	1(45°lb)		4 confusion
15°rb	21	4(30°rb)	9(0°b)	1(0°f)	1(45°rb)		
0°b	33	1(0°f)	1(15°lf)	1(15°lb			
15°lb	24	3(30°lb)	5(0°b)	2(0°f)	1(15°lf)	1(15°rb)	
30°lb	23	7(45°lb)	4(15°lb)	1(75°lf)	1(0°b)		
45°lb	23	3(60°lb)	8(30°lb)	2(75°lb)			42 forward
60°lb	13	8(75°lb)	10(45°lb)	4(90°l)	1(75°lf)		30 backward
75°lb	16	14(90°l)	2(60°lb)	2(75°lf)	1(60°lf)	1(60°rb)	6 confusion
90°l	24	7(75°lf)	4(75°lb)	1(60°lf)			
75°lf	20	9(90°l)	6(60°lf)	1(75°lb)			
60°lf	20	8(75°lf)	7(45°lf)	1(90°l)			
45°lf	26	7(60°lf)	2(30°lf)	1(75°lf)			45 backward
30°lf	24	9(45°lf)	3(15°lf)				22 forward
15°lf	22	10(30°lf)	4(0°f)				1 confusion

It is evident that the majority of the misplacements in the anterior quadrants are backward and in the posterior quadrants forward. In other words, there is a strong tendency to shift sounds away from 0°f and 0°b around toward the aural axis. In the front quadrants the sounds are shifted rearward, 100 backward and 37 forward; and in the rear quadrants they are shifted forward, 87 forward and 69 backward.

A possible explanation was thought to lie in the suggested overestimation of angular differences between directions, for the reason that in the nomenclature here used the two most prominent directions are 0°f and 0°b, and all other directions are designated as so many degrees to the right or to the left of these two reference points. For example, if a sound was given at 30°rf the question that at once arose in the observer's mind was, how many degrees is it from 0°f? And similarly in the rear quadrants 0°b served as the point of reference. Since the tendency was to shift the sounds away from 0°f and 0°b it seemed possible that it might be due to an overestimation of angular differences.

Another observation which seems to support this supposition is the fact that two of the observers, E. L. and C. P., show a decided tendency in the rear quadrants to shift the sounds rearward instead of forward as in the case of the other observers. This accounts for the fact that the predominance of the forward shiftings in the rear quadrants is not as great as the rearward shiftings in the front quadrants. In looking for an explanation of these two exceptions it was noticed that these two observers had a natural tendency to use 90°r and 90°l as the two points of reference for the localizations in the rear quadrants instead of using 0°b as the other observers did, while in the front quadrants they used 0°f, just as the others did. If, for example, a sound appeared to be at 60°rb they would almost invariably say that it was 30° back of 90°r. Since this method of designating directions was not contrary to any specific conditions of the tests the experimenter did not object to it. Now the thing to be noted is that if 90°r and 90°l were their points of reference for the rear quadrants the overestimation of the angular difference between these points and any given direction in these quadrants would tend to shift the sounds backward.

In order to determine this more specifically the tests for the rear quadrants were repeated with these two observers. They were told that they should always locate the sounds with reference to 0°b and not with reference to 90°r and 90°l. The number of determinations is the same as in the original tests so that the results are directly comparable.

E. L. made 8 forward and 10 backward misplacements in the original tests. The suggestion to change the method of designating the directions seems to have been successful at least to the extent of having approximately the same number of misplacements in each direction. C. P. made 3 forward and 25 backward misplacements as compared with 7 forward and 22 backward in the original tests. Here there is no essential change in the results. The suggestion to change the method does not seem to have been sufficient to overcome the more natural method of designation.

Some special experiments were planned to test in a more crucial way the possible overestimation or underestimation of angular differences between directions. Several small angles were chosen in two regions, in front and on the side, as follows: 3°, 5°, 10° and 15° in front with 0°f as the standard; and 10°, 15°, 20° and 30° on the side with 90°r as the standard. These experiments were made with the sound perimeter, using the telephone sound as stimulus.

The method of average error was followed. For instance, to obtain estimates of the angle 5° in the anterior region, the experimenter gave the stimulus at 0°f, then moved the receiver 5° either to the right or to the left and there gave the sound again in the same way, and then moved the receiver back to the original position. The observer was then told to open his eyes and by means of a pointer push the receiver from 0°f to the point where he thought the second sound was. Then the reading in degrees was taken and recorded. The observers were told that the angles varied in size but did not know the size of the angles, nor that a definite series of angles had been chosen, nor were they told of the nature of their judgments until the tests were finished.

Table VIII gives the results. Each observer gave twenty estimates of each angle, total 440. The figures in the table are percentages of overestimation or underestimation, plus meaning overestimation, and minus underestimation.

The results are decisive. The angles in front are considerably overestimated and those on the side are decidedly underestimated. There are only two exceptions and these are small.

TABLE VIII.

	0°f				90°r			
Angles	3°	5°	10°	15°	10°	15°	20°	30°
G. K.	60.	54	33.	32.2	..	−22.	−22.	−22.5
P. K.	26.	20.5	4.	−1.5	−45.	−38.5	−36.5	−11.5
P. L.	..	55.	16.	4.8	5.5	−16.6	−25.7	−34.3
Averages	43.	43.2	17.7	11.8	−19.8	−25.7	−28.1	−22.8

The angles chosen for the region in front are smaller than those used on the side for the reason that our discrimination between directions is there much finer.[1] The ratio is about 1 to 4, so that a difference of 3° in front is as easily perceived as 10° or 12° at the side. Two places in the table, one under 3° and the other under 10°, are blank because these differences were too small for those observers to detect with certainty and so no estimates were obtained.

It seems quite probable that this difference in discriminative ability in these two regions accounts for the overestimation and underestimation. To put it boldly, we overestimate in front because we can discriminate between smaller angular differences in direction than we think we can, and we underestimate on the side because we do not discriminate between as small angular differences in direction as we naïvely suppose. It frequently occurred during the course of these tests that in testing, for example, the angle 10° in the lateral region, the observers would say that the difference is very small and then they would move the receiver perhaps 7° or 8°, whereas in testing the same angle in the frontal region the difference would seem considerably greater and consequently the receiver would be moved 13° or 14°. This illusion may appropriately be called the 'auditory small angle illusion.' The overestimation and the underestimation seem to be primarily central processes and not peripheral.

[1] See Part I, Figure 3, p. 10.

SERIES V.

Monaural Localization of Sound.

Monaural localization presents various problems whose solution would throw considerable light upon binaural localization. In the experiments on the monaural localization three classes of observers were employed: (a) Persons in whom monaural conditions were produced artificially, (b) one person whose left ear was partly defective, and (c) two persons who had been completely deaf in one ear for many years.

Artificial Monaural Conditions.

The accuracy of discrimination between directions was investigated. The directions, 0°f, 45°rf, 90°r, 45°rb and 0°b in the horizontal plane through the aural axis served as standards. The modified form of the method of right and wrong cases which has been adopted for all the discrimination tests throughout this investigation was also followed here. The telephone was used as stimulus.

The tests were first made upon two observers in whom monaural conditions were produced artificially by closing the left ear by means of inserting a finger firmly in the meatus of the ear.[1] The intensity of the stimulus was so adjusted that when both ears were thus closed the sound could not be heard at all. The figures in Table IX represent in degrees the smallest differences that could be perceived between directions.

Table IX.

	0°f	45°rf	90°r	45°rb	0°b
C. E. S.	5.8	14.3	17.4	12.0	9.5
D. S.	6.6	7.3	14.5	7.3	11.6
Average	6.2	10.8	16.0	9.7	10.6

[1] The main objection to this method of closing the ear is that the circulation of the blood becomes quite noticeable in the closed ear, and consequently disturbs the attention. But this method was preferred to bandaging because it was almost impossible to bandage one ear sufficiently to exclude the sound.

Twenty-five determinations were made for each direction by each observer. The closing of the left ear considerably lessens the accuracy even on the right side.[1]

The tests were then continued on the left side and the first direction tested was 45°lb. The surprising result was that the observers were wrong in almost every answer. They felt sure that they clearly noticed a difference between the directions and yet their answers were consistently reversed in nearly every case. It was thought that this might be due to some peculiar reflection from the walls of the room. The experiments were, consequently, repeated out of doors on four observers, giving the following results:

TABLE X.

	0°f	45°rf	90°r	45°rb	0°b	45°lb	90°l	45°lf
F. O. S.	4.8	14.3	13.6	8.6	4.8	14.3	20.0	21.8
H. S. B.	3.4	29.0	14.3	12.0	2.9		20.0	7.5
D. S.	4.8	19.0	4.1	3.4	7.3		16.0	8.0
G. P. K.	3.4	8.0	8.0	10.2	4.0		23.3	5.8
Averages	4.1	17.6	10.8	8.5	4.8	(14.3)	19.8	9.0

Twenty-five determinations were obtained from each observer for each direction. The results are very similar to the indoor tests. Three places are left blank under 45°lb because there the same characteristic reversals occurred that were noticed in the indoor tests. None of the observers except D. S. knew anything of this observation before the experiments. Evidently it was not due to reflection of the sound from the walls.

The explanation undoubtedly is that under normal conditions when both ears are in use a sound in front of the standard, 45°lb, seems stronger than one back of it. But when the left ear is thrown out of activity the sound that then seems stronger is the one back of 45°lb, but according to the habitual method it would be located in front of the standard, and the outcome is the reversing of the actual positions of the stimuli because of the reversal of the data of localization. Similar reversals might

[1] Cf. Figure 3 in Part I.

be expected to occur in the left front quadrant but for unknown reasons they did not occur.

One observer did not reverse his answers but made the same observation as the others. Instead of placing the stronger sound in front of the standard he placed it back, evidently for the reason that he was conscious of the fact that the left ear was closed and consequently he thought the stronger sound must be the one nearest to the right ear. The introspection of another observer on the same point corroborates this and clears up the matter of reversals. He remarked: "The stronger sounds I call forward and those weaker back. [This of course reversed the actual positions.] That is the way they seemed when I had my attention on the left ear, but if I placed my attention on the right ear, that is, if I made myself conscious of the fact that I was using only the right ear, I would tend accordingly to correct myself and call the weaker ones forward and the stronger ones back." The other observers had no suspicion of reversals and assumed that most of their answers were correct.

In the next group of tests the aim was to determine the accuracy of localizing the twenty-four standard directions in the horizontal plane through the aural axis according to the method described in Series III. Table XI contains the results.

TABLE XI.

Monaural.

Number correct on the right side: 11 out of 44, or 25%	63.6%
Number on right side misplaced 15°: 17, or 38.8%	
Number of confusions on right side: 4	
Number correct on left side: 3 out of 44, or 6.8%	22.8%
Number on left side misplaced 15°: 7, or 15.9%	
Number of confusions on left side: 12	

Binaural.

Number correct on both sides: 32 out of 88, or 36.4%	81.2%
Number on both sides misplaced 15°: 43, or 44.8%	
Number of confusions on both sides: 10	
The distribution of cases was about equal for the two sides.	

These tests were made on four observers, each one going through the series of twenty-four standards, once monaurally

and once binaurally in order to compare the two. In the monaural tests the left side has a much smaller percentage of correct or approximately correct localizations than the right side—22.7% against 63.6%. It has also more confusions—12 against 4. In the binaural tests 81.2% are correct or approximately correct as compared with 63.6% on the right side in the monaural tests, showing that the exclusion of the left ear by artificial means affects the right side to a material extent.

One Ear Partly Defective.

The same two sets of tests were made upon an observer whose left ear had been defective for several years. Table XII gives the results of the discrimination for directions, and Table XIII for the localization of the twenty-four directions.

TABLE XII.

	0°f	45°rf	90°r	45°rb	0°b	45°lb	90°l	45°lf
Out of doors	4.0	14.5	19.0	29.0	5.8	25.3	17.0	12.0
In doors	7.3		14.5		8.0			

TABLE XIII.

In doors. Right side.	Out of doors. Right side.
Number correct: 1 out of 11	Number correct: 2 out of 11
Number misplaced 15°: 4	Number misplaced 15°: 1
Number of confusions: 1	Number of confusions: 5
Left side.	Left side.
Number correct: 1 out of 11	Number correct: 0
Number misplaced 15°: 2	Number misplaced 15°: 4
Number of confusions: 1	Number of confusions: 1

The localization is poorer than for the artificially monaural observers, particularly in the results of Table XIII. But there is practically no difference between the two sides, which can be accounted for by two reasons. First, the actual difference in acuity between the two ears was not as great as the observer thought. The threshold as found by the audiometer was 22.6

for the right ear and 37.9 for the left ear. Second, the observer had been defective for a number of years and had accustomed himself to the difference, whereas in the case of the artificially monaural observers the change was sudden.

Completely Monaural Observers.

The two observers, O and B, employed in these experiments were completely deaf in the left ear. Most of the data which are presented were obtained from O, a university student who has been deaf in his left ear since infancy but whose right ear has normal acuity, 16.7 as measured by the audiometer.

B is a middle aged physician who has been deaf in his left ear for thirty-two years. The deafness was consequent upon cerebro-spinal meningitis. Careful diagnosis by an otologist

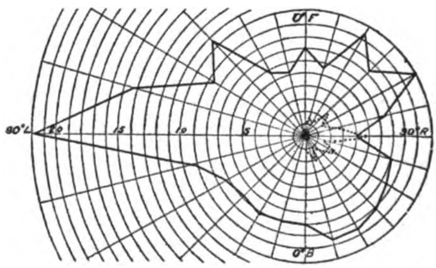


FIG. 9

showed that there is a lesion in the left auditory nerve. Otherwise the mechanism of the ear seems to be in perfect condition. The data obtained from this observer are in Table XIV.

Discrimination for directions—The first series of tests had in view the measurement of the discriminative ability between directions. The same method that has been followed in all the discrimination tests was followed here. The measurements were made out of doors.[1] The curve in Figure 9 contains the results obtained from O. It is based upon 1200 judgments, fifty for each of the twenty-four directions in the horizontal plane through the aural axis.

[1] The apparatus was a simplified form of the sound perimenter, consisting of only one arm and a scale. This was more convenient for outdoor experiments than the perimeter.

The main facts embodied in the curve are: (a) The most accurate region is at 90°r, just opposite the intact ear. A change in direction of four degrees can be detected there. (b) The most inaccurate region is at 90°l, opposite the deaf ear. A change of twenty-two degrees is necessary there to be noticeable. (c) In all the other directions the accuracy of localization is practically the same.[1]

This curve may be regarded as the typical monaural curve, comparable with Figure 3 in Part I as the typical binaural curve, which is represented by the dotted line in this figure.

There are, however, several interesting contrasts between monaural and binaural localization. In binaural localization 0°f and 0°b are the most accurate regions, and 90°r (or 90°l) is the poorest. In monaural localization, on the other hand, 90°r (or 90°l, in persons deaf in the right ear) is the most accurate region, while 0°f and 0°b are very much poorer. The accuracy at 90°r is about the same for a monaural as for a binaural person. This corroborates the statement[2] that in the vicinity of the aural axis the perception of direction depends almost entirely on one ear even in a binaural person.

While the monaural curve reveals some very interesting features and presents a remarkably symmetrical appearance, there is really nothing surprising about it. Its form might almost have been predicted on the basis of the facts known about binaural localization. We should expect the keenness of localization in the region near the intact ear to be the same in monaural as in binaural localization, because there the coöperation of the two ears is at a minimum. We should expect this to be the most accurate region because it is nearest to the active ear. Localization on the side of the deaf ear must necessarily be crude and unreliable because the sounds do not reach the intact ear directly. We should also expect the monaural localization to be much inferior to binaural localization in front and in the back, be-

[1] A few skirmishing tests on B revealed the same facts. His localization at 90°r was quite accurate, fully as accurate as in a normal individual. A change of five degrees could be detected. On the left side localization was quite poor.

2 See Part I, p. 24.

cause the great accuracy in those directions in binaural localization is due to the maximum coöperation of the ears.

What are the data upon which monaural perception of direction depends? Both observers were careful in their intropsective analysis and agreed closely in their statements. According to observer O, from whom most of the introspective accounts were obtained, the perception of direction depended upon two factors, viz: intensity and his 'sense of direction.' When he was asked why he located a sound in a particular place he would generally attribute it to greater or less intensity than the sound had when it came from another direction. If intensity did not seem to be the means of recognizing the direction, which was seldom, he would reply that he did not know just why he located the sound at that place. And if he was further urged to analyze the basis of his judgment, he would simply reply that his 'sense of direction' told him that it was there. The term 'sense of direction' is undoubtedly an abbreviated expression for a number of unconscious elements, perhaps mostly qualitative characteristics, which were so interwoven in the perceptive process through habit and experience that the observer was unable to disentangle them.

The intensity factors were by far the most important, in the estimation of the observer, but probably not as important as he considered them to be. It is, however, true that in his localizations he seemed to depend very consistently upon the variations in intensity.

He had a sort of system of relative intensities which may be described as follows. There were two poles, one of maximum and the other of minimum intensity, located on opposite sides of the head. The pole of minimum intensity, at which the sound seemed weakest, was at 90°l, opposite the deaf ear; and the pole at which the sound seemed strongest was apparently not at 90°r but just a little in front of it, at 75°rf. All other directions in the circumference between these two poles were distributed according to a regular scale of diminishing intensities from right to left. The observer's judgments seemed to depend almost wholly upon these intensity differences. In determining the position of a sound with reference to the stand-

ard he would locate it on the right if it seemed stronger than the standard, and on the left if it seemed weaker. In all the standard directions from 75°lf to 60°rf in the anterior quadrants, and from 75°lb to 90°r in the posterior quadrants, the sounds on the right of the standard seemed stronger than those on the left of the standard. At 90°l where a sound in front of the standard would have the same intensity as a sound an equal distance back of it, the discrimination between the directions was very crude. Both the one in front of the standard and the one back of it seemed stronger, and consequently it was almost impossible to distinguish between the two. At the pole of maximum intensity on the right side the same conditions would obtain, but it seemed that the qualitative data were more conspicuous in the perception of direction.

In a normal binaural person the monaural process is narrowly limited to the immediate vicinity of the aural axis. The monaural and binaural curves show equal accuracy only at 90°r. At 75°rf and 75°rb monaural localization is poorer.

The results of Bloch,[1] who has made similar experiments on the discriminative ability in the horizontal plane through the aural axis, are different. The only respect in which the curve in Figure 9 agrees with Bloch's curve is in the greater accuracy on the side of the active ear. Otherwise his curve is rather irregular.

The difference between the two curves is probably due to two reasons. In the first place, Bloch's observer was not strictly monaural. Monaural conditions were produced by closing the left ear. This does not insure absolutely monaural localization, and the sudden change by bandaging one ear does not give sufficient time to the individual to adjust himself to the new situation. The results in Table XI obtained under artificial monaural conditions, are quite different from the results of Figure 9. The observer upon whom the experiments here reported were made, had been deaf in the left ear since infancy. Secondly, Bloch's curve is based on only one third as many measurements as the curve in Figure 9.

[1] Bloch, "Das Binaurale Hören," 35.

Localizing the twenty-four directions—The twenty-four standard directions were given in chance order and the observer located the sounds. Table XIV contains the results.

TABLE XIV.

Positions of sound.	Judgments O		Judgments B	
0°f	15°lf	15°lf	30°rb	45°lb
15°rf	30°rf	30°rf	15°rf	15°rf
30°rf	15°rf	15°lf	60°rf	60°rf
45°rf	30°rb	30°rf	45°rf	75°rb
60°rf	45°rb	30°rf	45°rf	60°rb
75°rf	30°lf	45°rf	45°rf	60°rf
90°r	75°rb	90°r	90°r	45°rf
75°rb	75°rb	60°rf	60°rb	0°b
60°rb	45°rf	45°lf	90°r	90°r
45°rb	30°rb	30°rb	45°rf	60°rb
30°rb	45°lf	60°lf	90°r	30°rb
15°rb	30°rb	75°rb	45°rb	75°rb
0°b	15°rb	15°rb	30°lb	45°rb
15°lb	0°b	45°lf	0°b	45°lb
30°lb	45°lb	30°rb	0°b	0°b
45°lb	45°lb	15°lb	45°lf	30°lb
60°lb	30°lb	30°lb	45°lb	60°lf
75°lb	45°lb	30°lb	90°l	45°lb
90°l	60°lb	0°b	45°lb	45°lf
75°lf	75°lf	90°l	90°l	90°l
60°lf	60°lf	45°lb	45°lb	90°l
45°lf	90°l	45°lb	60°lf	60°lb
30°lf	60°lb	45°lb	45°lb	45°lb
15°lf	15°rb	75°lb	30°lf	45°lb

Total correct on right side: 7 or 15.9%
Misplaced 15° on right side: 12 or 27.3%
Confusions on right side: 6

Total correct on left side: 3 or 6.8%
Misplaced 15° on left side: 11 or 25.0%
Confusions on left side: 9

Total correct: 10 or 10.4%
Total misplaced 15°: 27 or 28.1%

The contrasts between the right and the left side, and between monaural and binaural localization, are clearly brought out.

The right side has more correct localizations (15.9%) than the left side (6.8%). The number misplaced 15° is nearly the same on both sides. The left side has more confusions, 9, than the right side, 6, implying that the localization is less reliable on the left side.

Comparing these figures with the same tests on normal binaural observers (upper part of Table VI), we notice that binaural localization is considerably better. In the latter 93.7% are correct or misplaced 15° as compared with 38.5% in the former. Another significant contrast is the fact that monaural localization has numerous confusions of the right with the left hemisphere. For example, 75°rf was placed at 30°lf. O made seven such confusions in 48 trials, while such confusions scarcely ever occur in a normal person. Only two such cases occurred in 864 trials, Table VI.

The monaural observers were also much more reluctant in giving their judgments. Many trials had to be repeated several times, which was seldom necessary with the binaural observers.

The data of localization seemed to be the same as in the foregoing experiments, both observers agreeing that intensity was the most potent factor. When a sound seemed weak it was placed somewhere on the left side, and when it seemed strong and clear it was placed on the right side. The observers depended largely upon the relative intensities of the successive stimuli. On the left side the sounds seemed fainter and farther away, while on the right side they seemed stronger and clearer. O frequently said, "This sound is stronger than the one before, it must be on the right side," or "This sound is fainter, it must be on the left side." B stated when the sound was given at 60°lb, "This is a good deal farther away than the preceding sound" (which had been at 45°rf). He located it at 45°lb.

Elimination of intensity—Since intensity seemed to play such a large rôle in the perception of direction, a special set of experiments was planned to determine how important a factor it was. An attempt was made to eliminate the characteristic intensity differences. The telephone stimulus of the sound perimeter was carried through the audiometer by means of

which the intensity could be accurately controlled. By a few preliminary trials a stimulus of intensity 35 (audiometer scale) at 90°r was judged to be equal to intensity 38 at 90°l. On this basis the stimuli from the directions between these extremes were graded, and the intensities were distributed accordingly among the directions as follows:

From 45°rf to 45°rb intensity 35 was used.
From 30°rf to 0°f and from 30°rb to 0°b intensity 36 was used.
From 15°lf to 45°lf and from 15°lb to 45°lb intensity 37 was used.
From 60°lf to 60°lb intensity 38 was used.

The observer was told that the strength of the sound would be different in different directions. Each of the twenty-four directions was given twice in chance order, yielding the following results. Of the forty-eight trials only three were correct, and eleven misplaced 15°, that is 29.2% were correct, or approximately correct, while in the same tests with uniform intensity, Table XIV, O had 41.6% correct or approximately correct.

Then the "discrimination" tests were repeated on Observer O at 45°rf, 45°lf, 45°lb and 45°rb. In each of these directions 100 trials were made, 50 in which the intensity (35) was unchanged, and 50 in which three intensities (33, 35 and 37) were used in chance succession. For example, the intensity at the standard direction might be 35 and the intensity at the side might be either 33, or 35, or 37.

TABLE XV.

45°rf	45°lf
Difference 10°	Difference 10°
Intensity uniform	Intensity uniform
74% correct	86% correct
Intensity varied	Intensity varied
64% correct	78% correct
45°lb	**45°rb**
Difference 15°	Difference 10°
Intensity uniform	Intensity uniform
84% correct	78% correct
Intensity varied	Intensity varied
58% correct	76% correct

These measurements clearly demonstrate that intensity is a very important factor in localization. When the intensity was varied so that the observer could not rely upon a certain intensity to indicate a certain direction, the localization became considerably inferior—on the average 12%. But intensit probably did not play as important a part as the observers thought. Otherwise, the localization, in which the different intensities were used, should have been even more inferior. The fact that the observer was able to localize with some certainty the sounds of varying intensities, indicates that qualitative factors played a considerable part.

GENERAL CONCLUSIONS

The results and conclusions may be stated under three heads, (a) a summary of the specific facts demonstrated by the experiments of Part II, (b) a summary of the factors which enter into the localization of sound, and (c) the bearing of these results upon the traditional theories of localization.

Summary of the Results in Part II.

1. The threshold of hearing is considerably lower, that is, sensitivity is keener, at the side than either in front or in the back.

2. There are two types of observers: Those whose threshold is lower in the front than in the back, and those whose threshold is lower in the back than in front.

3. The ratio of sensitivity of the side to front (or back) is the same irrespective of the absolute threshold. This ratio is approximately 3:4.

4. The fact that the threshold is lower at the side than in front or in the back means that a given sound will seem nearer and more intense at the side than in front or in the back because it is relatively so much higher above the threshold. In other words, the nearer a sound is to the aural axis the stronger and clearer it seems, and this apparent change of intensity with change in direction is a potent factor in the localization processes.

5. Pitch discrimination is decidedly poorer at the side than in front or in the back.

6. Discrimination for intensities of a sound is about uniform for all directions.

7. The richer and more complex a sound the more accurately it can be localized.

8. A pure tone, so far as it has been approximated, can be localized, although with much less accuracy than a complex tone.

9. Angular differences between directions are overestimated in the frontal region, and underestimated in the lateral region.

10. Monaural localization is considerably inferior to binaural localization. The most accurate region is opposite the intact ear, and the most inaccurate region is opposite the deaf ear. The nearer a sound is to the active ear the stronger and clearer it seems.

Summary of the Factors in Localization.

The experimental evidences gained in this entire investigation warrant, I believe, a division of all the perceptual data involved in the localization of sound into two classes, namely, intensity and quality.

I. INTENSITY.

The intensity factors are again of two kinds:

1. The Binaural Ratio of Intensities.

The localization of sound depends to a marked extent upon the relation between the intensities with which a sound strikes the two ears. (See Part I, p. 15.) Some of the experimental evidences for this are:

a. The accuracy of localization is greatest where slight changes in this ratio are most readily perceived, that is, in front and in the back.

b. Localization is poorest where changes in the ratio are not so easily perceived, that is, on the sides, in the region of the aural axis.

c. The presence of confusion points or directions in symmetrical positions for which the ratio is the same.

d. The difficulty of median plane localization, which is really a particular case of confusion points. The ratio is the same for all directions in the median plane.

e. The inferiority of monaural localization in which the ratio is entirely absent.

2. *The Monaural Ratio of Intensity.*

There are systematic differences in the intensity of a sound when it comes from different directions. Evidences for this are:

a. The threshold measurements (Part II, Series I), showing that the threshold is lowest in the region of the aural axis and highest in front and in the back, consequently a sound at the side seems stronger than in front or in the back.

b. The "distance" tests (Part I, p. 21), showing that a sound is estimated to be nearest in the region of the aural axis.

c. The introspections of the observers in all the localization experiments, and especially in Tables I to III and V to VII in Part I.

d. Monaural localization depends to quite an extent on the relation of the intensities for different directions, which is brought out distinctly by the introspections of the observers, and by the tests in which stimuli of different intensities were used, resulting in poorer localization.

e. In the artificially monaural tests the uniform reversing of the responses at 45°lb indicates the force of the variation of intensity with direction in the perceptive process.

II. QUALITY.

The localization of sound depends to a considerable extent upon the quality and complexity of the sound. The evidences for this are:

a. Complex sounds, such as the human voice, noises and the telephone sounds, can be localized much more accurately than

(relatively) pure tones, such as the singing flame and resonator tones. (Part II, Series III).

b. The introspective accounts of the observers testify to the importance of qualitative changes and signs for different directions. (See Tables I to III and V to VII in Part I.)

c. The fact that median plane localization is possible to some extent.

d. In monaural localization the elimination of the intensity factor still left a considerable accuracy in localization, which must have been due to the qualitative data.

The Bearing of the Results on the Theories of Localization.

Of the four or five theories[1] which have been advanced at various times, the intensity theory has no doubt received the most support. It attempts to explain the localization of sound essentially by the binaural ratio of intensities. In the light of present results it is evident that this ratio can not alone account for the perception of direction. In fact it plays a relatively small, though significant, part in the complete process.

The quality and complexity of sound are real, potent factors in the localization process.

Intensity itself has been demonstrated to be effective, not merely in the binaural ratio, but in the characteristic changes and differences in different directions.

If localization depended entirely upon the binaural ratio, monaural localization would be an impossibility; and binaural localization itself would be considerably inferior. We would be surprisingly deceived by the systems of confusion points. Theoretically there are numerous planes parallel to the median plane in which the ratio is the same for all points in the same circumference in a given plane. But the other intensity characteristics and the qualitative factors come in to render accurate localization possible.

The traditional intensity theory is in the main correct, but it is quite inadequate. We must add to it the qualitative ele-

[1] Pierce, "Studies in Space Perception." p. 52.

ments and the monaural quantitative elements. These two have coördinate value with the binaural ratio, in the auditory perception of direction. With these additions, the theory would be more appropriately called the "intensity-quality theory."

ON THE TRANSFERENCE OF TRAINING IN MEMORY.

GEORGE CUTLER FRACKER, A.M., PH.D.

CONTENTS.

Researches in the transference of training, or "spread of ability," seek to determine the influence of practice in one activity upon our abilities in other activities. There are, in general, two methods by which this problem has been attacked—the method of correlation, and the method of direct experiment. The method of experiment was chosen for this study. The usual means of conducting research by the experimental method in the transference of training is to employ two sets or series of experiments; one set to be given before and after training; the other a training set, in which one may be practiced for some time. The experiments given before and after training may be called the test series, and the other set, the training series. The test series is made up of several experiments, some of which are like the training series, while others differ.

The object of these two sets of experiments is to discover what effect practice in the training set has upon the test set, in

which the observer is not trained. This effect is measured by the difference in the results between the test series given before the training, and the test series given after the training. In order to measure the amount of training in the test series itself, two sets of observers are used: one set who take both the test and training experiments and another set who take the test experiments only. The difference in the gain between those trained and those untrained indicates the influence of the training experiments. The observers composing these two sets are selected on the basis of similarity in age and ability.

The present research is devoted to a study of certain aspects of memory. The training series consisted in practice in memory for the order of four tones. The experiments of the training series and the manner of conducting them will be explained under "Training Series." The test series consisted of eight experiments, as follows:

1. Memory for poetry.
2. Memory for the order of four shades of gray.
3. Memory for the order of nine tones.
4. Memory for the order of nine shades of gray.
5. Memory for the order of four tones.
6. Memory for the order of nine geometrical figures.
7. Memory for the order of nine numbers.
8. Memory for the extent of arm movement.

The relation of the respective test series to the training series, in the above experiments, was as follows:

The four grays; different in content, same in method.

The nine tones: same content, different in method.

All the other tests: different in content and method.

This relation between the training and the test experiments was planned in order that the elements concerned in transference might be determined by analysis of the final results. In order to aid still further in this analysis, each observer was asked to write a careful introspection at the close of each day's training, after each experiment of the test series, and a general introspection at the close of the experiments giving his observa-

tions and conclusions concerning the essential elements in improvement and transference. The object of the introspection was to discover the method or lack of method of each observer. Every effort was made to guard against giving the observer any hints as to how to perform the memory part of the experiments. At the beginning of each test, observers were given written instructions describing just what they were expected to do in response to the stimuli, but conveying no information as to how to do it.

The test experiments present the following important advantages: (1) Each is a task sufficiently difficult to demand intense application while it lasts. (2) Each test is so brief that it affords a minimum amount of training within itself. (3) The time between the first and second tests is fairly long. (4) The tests similar to the training series were taken in double fatigue order. (This order is explained under "General Experimental Conditions.") (5) The test material is of such nature that the second test could be made exactly equivalent to the first test without being a repetition of it.

DESCRIPTION OF THE TEST SERIES OF EXPERIMENTS

Experiment I. Memory for Poetry. Two stanzas of "Eve of St Agnes." The observer memorized two stanzas of this poem so that he could repeat each aloud to the experimenter without error. A record was kept of the time taken for each stanza, and of the errors made.

Experiment II. Memory for the order of four shades of gray. This experiment consisted in exposing before the observer, numbers two, seven, thirty and forty-five of the Hering Grays, by means of the psychergograph. The psychergograph consists of two main parts, the stimulator and the recorder. As the recorder was not used in these experiments, the description of it is omitted here. The following partial description of the stimulator is quoted from the original description by Seashore, Univ. of Iowa, Studies in Psychology, III, p. 5.

"The stimulator is a plain case, 40 cm. square, with a slanting cover. Near the front edge of the cover is a signal window, 8 millimeters wide by 20 milli-

meters long, through which the signals are seen. One hundred signals are pasted or printed on a paper disk, 38 cm. in diameter, so that when the disk revolves they are seen singly in succession right back of the signal window. The paper disk is clamped on a metal wheel which has fifty teeth on its edge. This wheel is energized by a strong clock spring which revolves it and the disk one one-hundredth of a revolution, thus exposing a new signal every time the detent which holds it is released. This detent is in the form of a lever escapement and is operated by electro-magnets." * * *

"A circle of the revolving disk is seen through the cover. On this there is a cross line which passes before the circular scale of a hundred units and indicates to the experimenter which signal is in view. This indicator serves at once as a counter of the number of acts performed and as a guide for the beginning and ending of the series. The order of the signals is determined in the making of the series, either by chance or by some suitable system. The experimenter, therefore, knows the actual sequence of the signals in every series, but the observer has no means of knowing at any time what signal shall appear."

The stimulator was arranged to expose each of the four gray disks for one-half second, an interval of one-half second being allowed between each exposure. Each disk was seven mm. in diameter. After the four grays were exposed, a blank remained before the observer for four seconds, then another arrangement of the four grays was given and another blank exposed. When the second blank appeared, the observer responded, giving aloud the order of the first group of four grays. After the third group had been exposed, he responded to the order of the second group, and so on through the series. The series consisted of forty groups taken in double fatigue order. In responding to the order of the four grays, the observer called the darkest gray, 4; the next lighter, 3; the next lighter, 2; and the lightest, 1. The grays were given in twenty-two different mutations, but the orders 1, 2, 3, 4, and 4, 3, 2, 1 were not used.

Experiment III. Memory for the order of nine tones. The third set of stimuli used consisted of four tones, varying in intensity, and delivered to the observer through a telephone at the rate of one tone per second, each stimulus sounding one-half second. The four sounds were produced by placing a telephone receiver in circuit with a 100 v.d. electro-magnetic fork and branching the circuit through four lines of resistance so adjusted as to produce four readily distinguishable tones when

the four keys of these branches were closed in turn. The e. m. f. was kept constant. These four tones were arranged so as to form a group of nine tones. After the delivery of the stimulus group, an interval of nine seconds was interposed, during which the observer responded aloud to the order in which the nine tones had been given. The tones were called 1, 2, 3, 4, in the order of loudness, 4 being the loudest. The 1, 2, 3, 4 and the 4, 3, 2, 1 orders were avoided. Twenty groups of nine tones each were given in double fatigue order.

Experiment IV. Memory for the order of nine grays. In this experiment, the four grays of Experiment II, were so arranged as to form a group of nine grays. These were exposed on the stimulator at the rate of one per second; exposure, one-half second. After each group of nine grays had been exposed, there followed an interval of nine seconds, during which the observer gave aloud the order of the grays in the group just given. Twenty groups of the nine grays were given in double fatigue order.

Experiment V. Memory for the order of four tones. The four tones were those composing the major chord on the piano. Instead of responding by number, the observer responded by the names Do, Me, Sol, Do–2. The tones were produced in the same order, with the same duration, at the same rate and with the same response intervals as the grays in Experiment II.

Experiment VI. Memory for the order of nine geometrical figures. The nine figures drawn upon a card are described as follows:

> "Each figure is composed of three lines; the lines are all straight; two lines are equally long, and the third is half as long as these; the two long lines always adjoin each other; the lines join either at the end or in the middle; no line is crossed; no two figures are alike; the angles are right angles." [1]

The stimulus card was exposed ten seconds, and the observer was given one minute in which to reproduce all the figures he could remember, drawing them in the same relative positions and proportions as on the card. Five records were taken.

Experiment VII. Memory for the order of nine numbers.

[1] Seashore, "Elementary Experiments in Psychology."

The stimulus for this experiment was nine numbers of two figures each, read aloud at the rate of one pair each second and a half. After nine pairs had been read, the observer was given fifteen seconds in which to write as many of the pairs as he could remember. Ten sets of nine pairs made up the test.

Experiment VIII. Memory for the extent of arm movement. The apparatus for this experiment consisted of a glass rod mounted one-half inch above a metric scale. A hard rubber cylinder, about one inch in diameter, was fastened firmly at one end of the glass rod so that its edge tallied with the zero point of the metric scale. Upon this glass rod was a second hard rubber cylinder freely adjustable. The observer, with eyes closed, moved his finger with a free arm movement along the glass rod, from the fastened cylinder to the adjustable cylinder, held at a standard point by the experimenter. The observer was allowed to move the finger out and back twice. The experimenter then moved the adjustable cylinder away from the standard position, and the observer moved his finger along the rod until he thought he was reproducing the standard distance. Three standards were used; viz: fifteen, twenty, and twenty-five cm., and ten trials were taken in varied order for each standard.

DESCRIPTION OF THE TRAINING SERIES OF EXPERIMENTS

The apparatus for the training series consisted of that described in test Experiment III, the nine tones. The four intensities of tone of the fork were arranged in all possible combinations except the 1, 2, 3, 4, and the 4, 3, 2, 1, order. Enough of these combinations were used and repeated to make a series of seventy-five groups. The observer, seated comfortably at a table with a telephone receiver carefully adjusted to his ear, listened to the four intensities of sound. The experiment was carried on in exactly the same way as the four grays of the test series except that tones were used instead of grays and the number of groups extended to seventy-five, four sets of which made up a day's practice. It was possible to give seventy-five groups in about ten minutes. At the end of each ten minutes, a rest of one minute was taken.

For three observers, Tuesdays, Thursdays and Saturdays of each week were the practice days; for five, Tuesdays and Thursdays, or Wednesdays and Fridays, of each week.

METHOD OF RECORDING AND OF ESTIMATING RESULTS.

The observer's responses were kept by a recorder who used mimeographed sheets corresponding to the sheets used by the experimenters as a guide in giving the stimulus. These mimeographed sheets consisted of columns of numbers corresponding to the order of the numbers of the grays or tones used in the test and training series. The order in which the groupings were given was readily changed by beginning at different parts of the sheet. If the observer omitted to respond to any stimulus group, a line was drawn through that number upon the record sheet. If he responded incorrectly, his reply was written above the corresponding number on the record sheet.

Results in both the test and the training series are estimated on the basis of the per cent of correct responses. Training curves are plotted to show the per cent of correct responses in each of the four sets of seventy-five groups. Test results are shown by lines drawn across the charts.

GENERAL EXPERIMENTAL CONDITIONS.

Every observer was allowed a short preliminary practice at the beginning of each test or training period with the grays and the tones in order to secure adaptation. This seldom took more than a few trials.

No observer knew of the results of his records until after the experiments were entirely completed, with the single exception of G. C. F., during his second training series.

Every effort was made to preserve uniform conditions, especially for the two test series. A record was kept of the hour of the day when each experiment was taken; and the same day of the week, and the same hour of the day, were kept for each observer. Great care was used to keep the temperature, light, and sound conditions of the room as constant as possible. The

experiments were carried on in a room where the apparatus remained in the same position, and all the above elements could be easily controlled.

In order to determine the amount of improvement due to training, the observers were divided into two classes: (1) Those who took both the test and training experiments, and (2) those who took the test experiments only. Of the first class there were eight, and of the second class, four.

The following order was maintained in giving the experiments in the test series:

1. Poetry, two stanzas.
2. Four Grays, twenty groups.
3. Nine Tones, ten groups.
4. Nine Grays, ten groups.
5. Four Tones, twenty groups.
6. Geometrical Figures, five trials.
7. Nine Numbers, ten columns.
8. Arm Movement, ten trials for each of three standards, 15, 20, and 25 cm.
9. Four Tones, twenty groups.
10. Nine Grays, ten groups.
11. Nine Tones, ten groups.
12. Four Grays, twenty groups.

This arrangement gives a double fatigue order for the four experiments most closely resembling the training series.

The observers. G. C. F. is a teacher of psychology and has carried on experiments in this subject on two former occasions. With Professor Seashore, he devised the experiments of this series, set up the apparatus for the tests, worked out the scheme for the mimeographed record blanks, and served as experimenter and recorder for several of the observers. He is considerably older than the other observers. D. S. was also a trained observer and an instructor in psychology; thoroughly familiar with the material and method of this experiment. Nearly two years previously, he had been trained in experiments almost identical with this series[1] as regards stimuli and

apparatus, but the responses had been made by signals on four keys, instead of speaking. F. S. was a graduate student in psychology, trained in many forms of psychological experiment. E. M. C., H. C. E., A. R. F., M. M. M., and M. L., were college juniors with some experience in psychological observation. These observers took both the training and the test experiments.

J. W., M. C., M. D. F. and D. D. W. took only the test experiments; J. W. was a senior in college and somewhat older than the others. M. C., M. D. F., and D. D. W. were college juniors and all were familiar with psychological experiment and observation.

RESULTS OF THE TRAINING SERIES.

The practice curves and the curves of improvement in the test series are shown on Charts I to IX inclusive. All the observers except one were trained for four weeks, two or three days per week. G. C. F. was trained for eight weeks three times per week. Two charts, I and II, show the results for this observer.

Each point in the heavy curves represents the average for seventy-five groups of tones. Four such sets, of seventy-five each, represent a day's work. The curves are plotted on the basis of the per cent of correct responses. Each day's average is shown by a heavy horizontal line.

These curves show the progressive gain by practice in the training series and the relative ability in the end tests before and after the training.

G. C. F. thinks that the advantage he has had in his experience as experimenter is offset by the development of automatism which he was forced to break when he became observer. Before beginning the training, he had served as experimenter for three other observers, and had thus made about 10,000 reactions from the same stimuli but with attention upon the beat of the metronome, the delivery of the stimulus, etc., instead of upon the sounds of the stimulus. At the beginning of the training,

[1] SEASHORE AND KENT, "Periodicity and progressive change," Univ. of Iowa, Studies in Psychology, IV, p. 82 ff. (see practice curve opposite p. 85).

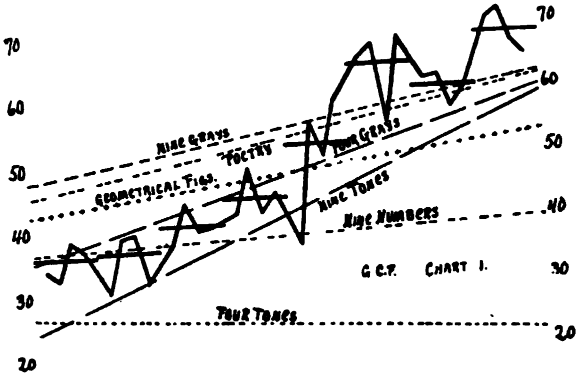


FIG. 1

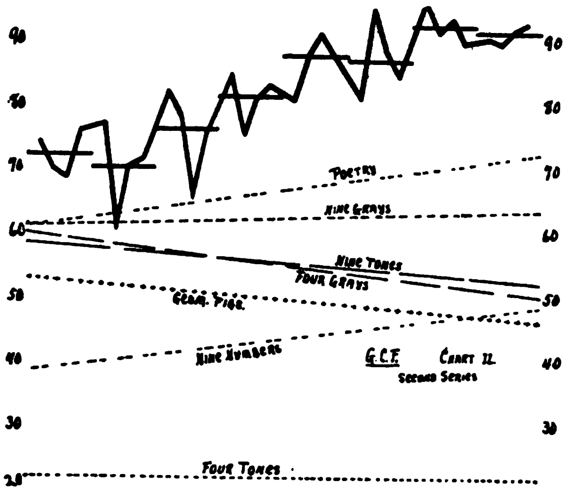


FIG. 2

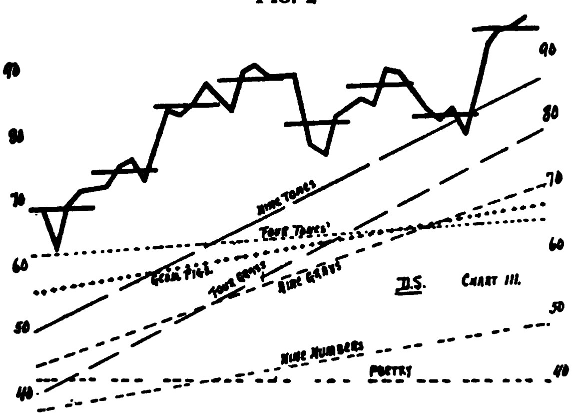


FIG. 3

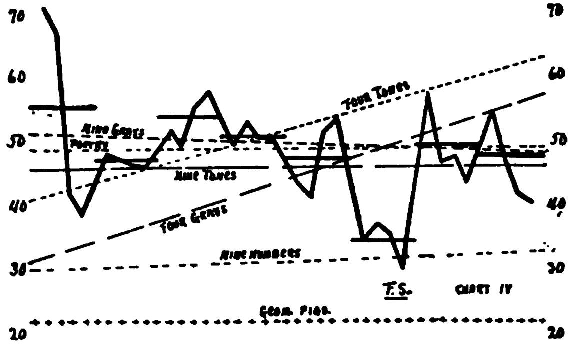


FIG. 4

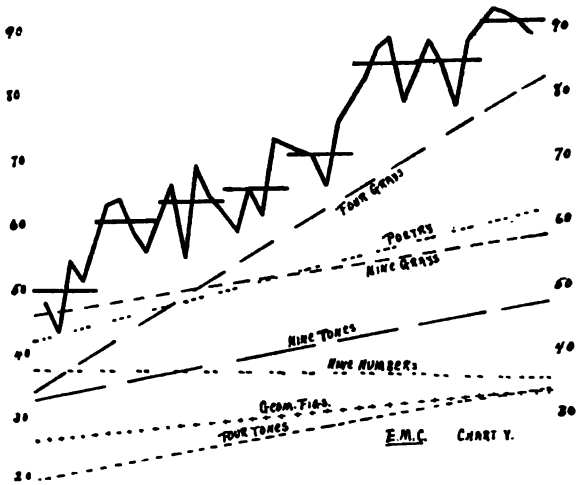


FIG. 5

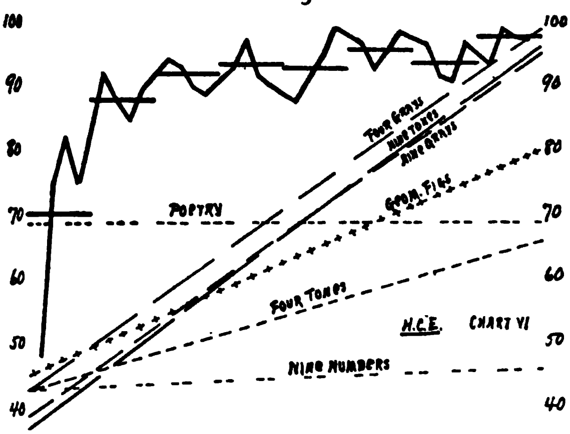


FIG. 6

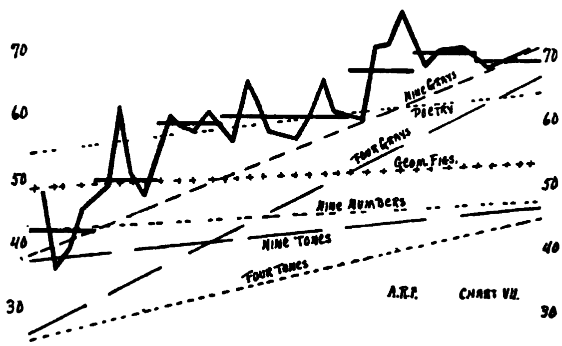


FIG. 7

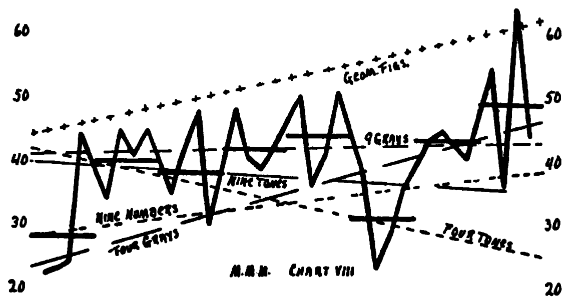


FIG. 8

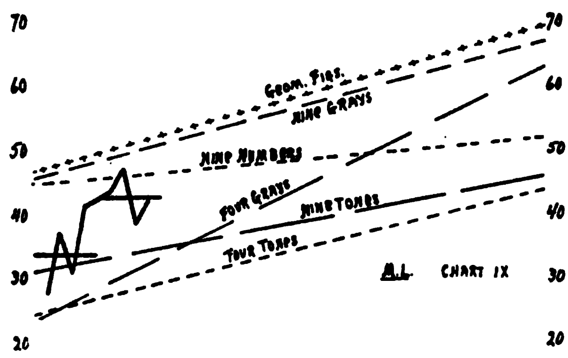


FIG. 9

this kind of response had to be broken up and the attitude of the experimenter interfered with that of the observer. Lack of familiarity with conditions of experiment and with apparatus is mentioned by other observers as a cause for the low starting point of the curves. Another reason given is the tendency to bring outside associations into the practice. Inability to hold the attention to the new task is also mentioned.

G. C. F. believes that his continued practice as experimenter during the training was a cause of fluctuation in his curve.

The progression in the curves ranges from a loss of 9 per cent to a gain of 48 per cent, or an average gain of 28 per cent. The average point at which the curves begin is at 50 per cent, and the average summit of the curves is 73 per cent. The range of the points of beginning is from 32 to 70 per cent, and the range of the summits is from 46 to 98 per cent. The rapidity of the rise is so closely connected with method that it will be discussed under that head.

The explanations offered for the fluctuations in the curves are mainly inability to hold attention, disconcerting associations, guessing at results, and lack of a method to which to adhere.

The introspections reveal the methods used by different observers. At the beginning of practice these introspections show that every observer did more or less guessing in responding. A number of devices were tried during the first days of training. All observers began by repeating as many times as possible, before the response, the order of the stimulus group. Several attempted to divide each group of four sounds into groups of two sounds each. Some tried to locate the loudest and the faintest sound in the group and remember the order of the other sounds by these. There were difficulties with all these methods. In the attempt to repeat the first group as often and as rapidly as possible observers found that, when the second group came, the repetitions had to cease while attention was given to the new stimulus. As a result the first group was lost in the attempt to fix the second, or in responding to the first the second was forgotten. Under such stress there is a strong tendency to guess, or to become confused. Both these things happened frequently. Several observers attempted to separate the stim-

ulus from the response. With G. C. F. this became a factor in his method. With F. S. there seemed to be a tendency to try new methods throughout the training. In guessing, some times the whole group is guessed at, other times one or two of the numbers of the group. All observers said that speaking in response disturbed the retention of the first group. After trying several schemes observers settled down to a definite way of retaining the groups and of making the responses. The individual methods adopted can be shown best by quoting from the introspections.

G. C. F: "The principal features of my method are: first, an imagery of position in space for the four tones. Number four is right at the ear, three is about four or six inches away from the ear, two is several feet away, and one is a faint sound a long distance off. The exact position of two and one is not clear, but the position of three and four is definite. Second, there is a separation in attention between the stimulus and the response, that is, attention is given to the first group, which is fixed by the imagery above described, and placed upon the tip of the tongue to be spoken immediately after the second group is heard. When the first group is thus delegated to the motor side, it is dismissed from active attention, which is then focused upon the second group. The second group is fixed almost while the first group is being automatically responded to."

With D. S. the method used goes back to his previous training. In the introspection following the first day's practice he said that many "names" used formerly came back during the hour and helped materially in the responses. The names referred to are the forms of imagery used by D. S. to identify different arrangements of the four sounds. On the second day of practice the system of "names came back completely," and he attributed practically all of his improvement to the recovery of his system. From thence he improved steadily.[1]

F. S. tried to remember the tones in groups of fours and to hold them by rapid repetition. After the second group was given he responded to the first group, and then immediately repeated the second group as often as possible before the next stimulus was given. Later he tried to remember the first two numbers of a group and guessed at the other two. When he missed a

[1] For full account of the method used by D. S. in his former training series see Seashore and Kent, op. cit. pp. 87–90.

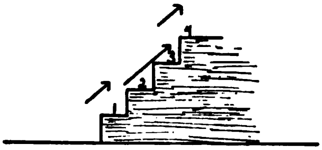
group altogether he guessed at it. Sometimes he grasped the first two numbers of a group with the last number and supplied the third. He found great difficulty in retaining one group while securing the next. Speaking in the response disturbed the retention of the second group. As practice proceeded he noticed no new method but seemed able to take things more quietly and to respond with less effort. He noticed especially a tendency to listen to the loud tone and to locate it in the group. This tendency results in a species of auditory imagery, but an imagery not definitely recognized. In the last introspections, the observer said that there was a tendency to locate the loudest and faintest tones in a group.

H. C. E: "In this training I used two methods of remembering. The first four numbers I repeated several times during the four seconds interval, and continued repeating them while the next four were being given; then I would give them aloud without giving any actual attention to them. While the second group was being given, I turned my active attention to it, first getting a visual image of their position and at the same time repeating the first four. In the four intervening seconds, I repeated aloud the first four members without any active attention to them, keeping my active attention on the visual image. Just as soon as the first four were repeated aloud and gotten out of my mind, I would immediately turn the visual image of the second group into a repeating image and wait for the next group of four. This same process was kept up throughout the experiment. The imagery is a vertical arrangement of the numbers of the four tones, in appearance somewhat like keys. The lowest number or key was 1, and the highest, 4. The keys were apparently numbered as though printed. Some groups, however, such as 4, 1, 2, 3, were divided into two, 4 being in a group by itself, and the 1, 2, 3 combined. The visual image is not an image of the numbers. They are arranged in vertical order and I pick them out vertically but do not see the numbers themselves. The one highest up or number four, is the largest, and number one is very small, with numbers three and two correspondingly between. As near as I can tell, they are round or disk-shaped and appear thus:

As practice continued the visual image became more familiar and found myself more and more able to remember the groups without giving much attention to them."

A. R. F.: "After repeating the numbers of the first group, I would form a sort of mental picture of how they would look on a scale, and this would help me in remembering them. When I got confused, I just stopped and did not

try to recall, but went on with the next group. The figure I find myself using after the third or fourth day of training is like the following:



There was a flight of four steps, each numbered in ascending order. This figure illustrates the group 2, 3, 1, 4; 2 and 3 being consecutive were represented by a long arrow and, because they came first, the arrow was close to the stairs. 1 came next and was a little farther away, and was a short arrow, being alone. 4, the last one, was still farther out, and was also a short arrow. I recalled the group by the position and direction of the arrows in the mental image. After listening to the second group, I go back and, from the picture, get the numbers in the order given. The mistakes I make are caused by taking too long to form the picture or to recall it."

M. M. M. developed a method of very vivid imagery rapidly. He says:

"In remembering these groups I thought of them as being in a position like four keys on a piano, such as C, D, E, F. I remembered them as 1, 2, 3, 4, and while one groupwas being given I tried to keep a picture of the order the keys took in the preceding group. It was much easier to remember the group when the numbers came together, such as 1, 3, 2, 4; in this case, I thought of 3, 2, as being between 1 and 4. I could see the keys just as if they were pressed down. In trying to remember a group, I sometimes hung to it too long and became confused in getting the next group. As practice continued this method became more reflex, and it was easier to remember the groups."

E. M. C. began the training by repeating to himself, about twice during the four seconds interval, the first group. Later the repeating became automatic, so also did the response; and he adds:

"I find that if I miss a group, that breaks into the rhythm and it is hard to get into the swing again. Sometimes I give a group and guess just to keep up the rhythm. During these tests I have been convinced that the subconscious does a great deal for us."

It seems that E. M. C. did not recognize a particular form of imagery as did the other observers. It is not clear from his introspections that he had an imagery form, nor is it clear that he did not have one. Throughout the introspections for the second test experiments, however, E. M. C. speaks of the aid

he received from the training series method in securing results in the test experiments. There seem some indications, therefore, that he had a method or some particular form with which he had become familiar during the training and which he used in the final test. This method must have been something more than that of repetition which he used in the first test experiments.

In the case of M. L. the training was carried on for two days only, and the introspections contain no definite statement concerning the specializing of method in the form of imagery. But under her introspection for the nine grays it is fully described. During the final test series she says that there was a decided help from the first test and the method of the training series. She says:

"The loudest tone seemed to serve as a sort of station around which the others grouped themselves. This scheme helped very much in securing correct results. Sometimes the lowest tone served as a station. Such combinations as 3, 2, 1, 4, or 2, 3, 4, 1, were much easier, because of my way of remembering."

In the matter of recognition of a method, or what seems to be the same thing, the recognition of an individual imagery, the observers did not all recognize a particular method to such an extent as to describe it in introspections. From what has been quoted it is apparent that all but two, F. S. and E. M. C., recognize a peculiar imagery; and the evidence shows that they also used imagery but did not recognize it as such. The relation which the recognition of the method or imagery bears to improvement is significant. Without a conscious recognition of the imagery an observer may improve rapidly, or slowly, but a steady improvement seems to follow if an imagery exists, and is consistently used. E. M. C. seems to illustrate this. F. S. illustrates an observer who had imagery but who failed to use it consistently. With observers who recognized imagery the rate of the improvement seems to coincide with its recognition. This is reasonable for, as observers say, the recognition of a method gives one confidence in his ability to do the work. With G. C. F. imagery was recognized about the fourth or fifth day, and the rise in the curve is most rapid immediately after.

With D. S., imagery was recovered the first and second days and the rise in the curve is most rapid during the first, second and third days. With H. C. E., imagery was recognized the first day and the gain is very rapid the first two days. With A. R. F., imagery was described on the fifth and sixth days and the gain is greatest on the sixth day, although the gain is great on the second day also. In the case of M. M. M., whose curve shows only a slight gain during practice, yet who recognized imagery on the first day, the rise is great on the first day but is not great thereafter until the seventh and eighth days when he began to recover from an attack of the grip, from which he suffered severely on the sixth day.

With A. R. F., whose imagery seems somewhat intricate, the relation between complexity and rate of improvement is shown. This observer speaks of losing many groups because he had not time enough to adjust his imagery to the group.

In the case of untrained observers, a recognition of imagery is not alone sufficient to give this confidence of improvement; for many say that they are not sure that they can use the imagery in other tests. A certain familiarity with imagery would seem, therefore, essential. This familiarity the training series gives.

The fact then seems to be that steady improvement may take place because of the use of an imagery without a conscious recognition of its presence. An imagery may even be recognized without adding essentially to the speed of improvement, but a recognition of it adds confidence in one's ability and reliance upon one's method, which is pretty sure to result in rapid improvement.

The essentials of method in training as brought out by these experiments are; first, familiarity with conditions of training, such as the room, the light, heat, furniture, apparatus, the experimenters, the adjustment of the observer to the apparatus, and learning what is expected of the observer; second, the use of rapid and frequent repetition in order to retain; third, the sorting out of things essential to the performance of the act from those things that are non-essential;[1] fourth, the selection,

[1] See Coover and Angell, "General practice effect of special exercise," Am. Jour. Psychology, XVIII, pp. 328–341.

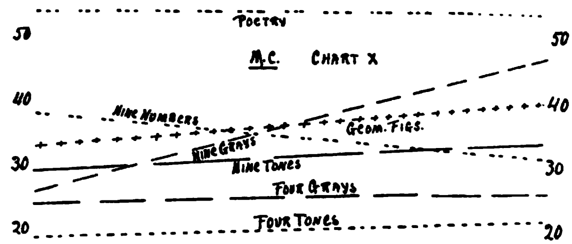


FIG. 10

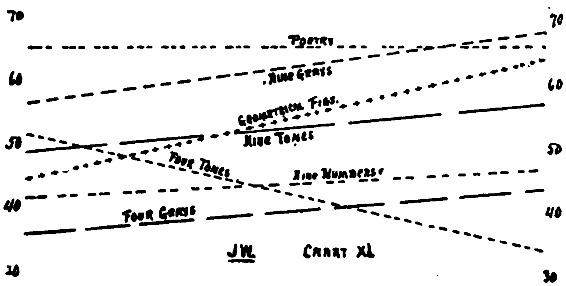


FIG. 11

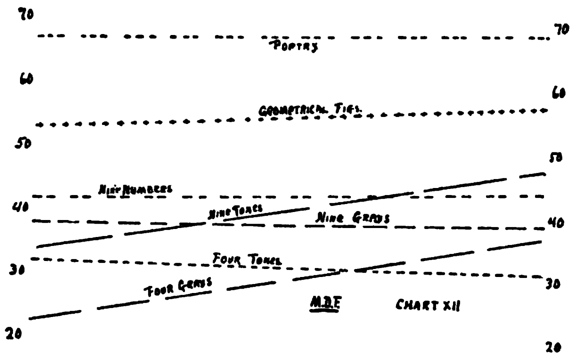


FIG. 12

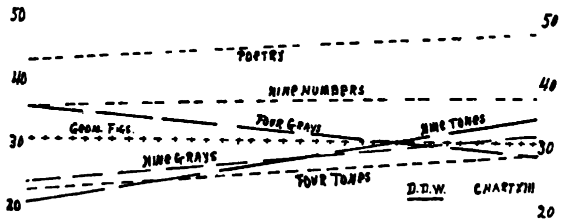


FIG. 13

TABLE I.

Results of Test Series for Trained Observers.

	G.C.F.						D.S.			F.S.		
	1″	2″	3″	2″—1″	3″—2″	3″—1″	1″	2″	2″—1″	1″	2″	2″—1″
Poetry												
1″ Stanza	43	61	67				36	39		44	45	
2″ "	49	62	77	+15	+11	+26	49	39	−3	55	54	0
Av.	46	61	72				42	39		49	49	
4 Grays												
1″ Trial	29	51	41				43	95		25	59	
2″ "	45	68	61	+22	−8	+13	39	61	+37	38	56	+26
Av.	37	59	51				41	78		31	57	
9 Tones												
1″ Trial	13	61	58				49	82		54	49	
2″ "	37	56	48	+34	−6	+27	51	91	+35	38	46	+1
Av.	25	59	53				50	85		46	47	
9 Grays												
1″ Trial	54	59	71				47	72		44	50	
2″ "	63	63	57	+2	+3	+6	42	68	+26	59	48	−2
Av.	59	61	64				44	70		51	49	
4 Tones												
1″ Trial	36	24	31				69	53		45	58	
2″ "	18	18	11	−6	0	−6	54	76	+3	38	69	+22
Av.	27	21	21				61	64		41	63	
Geom. Figs. 5 Trials	44	53	47	+9	−6	−3	56	67	+11	22	22	0
9 Numbers	37	39	49	+2	+10	+12	37	48	+11	30	34	+4
Movement												
15 cm.	96	95	95	−1	0	−1	95	94	−1	94	96	+2
20 cm.	97	97	98	0	−1	+1	98	95	−3	94	93	−1
25 cm.	98	97	98	−1	+1	0	97	94	−3	94	95	+1

1″, 2″, and 3″ = First, second and third tests.
2″—1″ = Average of First test minus average of the second test.
\+ = Gain.
— = Loss.

TABLE 1 (Continued)

	E. M. C.			H. C. E.			A. R. F.			M. M. M.			M. L.		
	1″	2″	2″—1″	1″	2″	2″—1″	1″	2″	2″—1″	1″	2″	2″—1″	1″	2″	2″—1″
Poetry															
1″ Stanza	41	56		54	65		61	70					89	97	
2″ "	44	66	+19	84	73	00	48	59	+10				92	100	+8
Av.	42	61		69	69		54	64					90	98	
4 Grays															
1″ Trial	10	83		24	98		25	73		15	48		19	69	
2″ "	59	83	+48	63	100	+56	29	60	+39	33	46	+23	30	56	+39
Av.	34	82		43	99		27	66		24	47		24	63	
9 Tones															
1″ Trial	27	47		23	94		31	43		30	36		28	38	
2″ "	40	50	+15	52	99	+59	44	48	+8	50	34	+5	33	54	+15
Av.	38	48		37	96		37	45		40	35		31	46	
9 Grays															
1 Trial	39	64		26	93		53	70		42	31		40	68	
2″ "	54	52	+12	53	98	564	23	71	+32	40	56	+2	52	67	+21
Av.	46	58		39	95		38	70		41	43		46	67	
4 Tones															
1″ Trial	20	40		33	63		30	44		41	32		29	43	
2″ "	19	29	+15	54	70	+23	20	46	+20	44	19	−16	23	45	+19
Av.	19	34		43	66		25	45		42	26		25	44	
Geom. Figs. 5 Trials	27	33	+6	45	80	+35	49	54	+4	44	62	+18	47	69	+22
9 Numbers	38	36	−2	43	46	+3	43	47	+4	29	38	+9	45	52	+7
Movement															
15 cm.	96	95	−1	98	96	−2	96	95	−1	97	95	−2	88	94	+6
20 cm.	98	96	−2	98	98	−0	95	96	+1	97	97	0	89	97	+8
25 cm.	97	97	0	97	96	−1	97	96	−1	97	93	−4	90	93	+3

TABLE II.

Results of First and Second Tests for Untrained Observers.

	M. C.			J. W.			M. D. F.			D. D. W.		
	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"
Poetry*	48	34	00	65	65	0	70	63	+2	40	51	+5
	61	74					64	75		47	45	
	54	54					67	69		43	48	
Four Grays	21	37	+2	32	41	+7	25	28	+15	36	23	−7
	28	14		40	46		20	46		36	35	
	24	26		36	43		22	37		36	29	
Nine Tones	30	30	+5	33	55	+9	28	43	+14	24	28	+14
	29	38		63	60		39	51		18	42	
	29	34		48	57		33	47		21	35	
Nine Grays	19	43	+21	48	64	+11	40	44	0	22	36	+8
	33	51		65	72		38	34		25	30	
	26	47		57	68		39	39		24	32	
Four Tones	20	21	+2	37	31	−17	29	28	0	18	28	+6
	18	21		66	37		36	36		28	30	
	19	21		51	34		32	32		23	29	
Geom. Figs.	33	40	+7	44	64	+20	53	58	+5	31	31	00
Nine Numbers	39	31	−8	41	47	+4	42	44	+2	36	38	+2
Movement	94	94	0	88	97	+9	97	92	−5	96	96	0
	97	96	−1	92	97	−5	98	95	−4	95	96	+1
	98	95	−3	95	97	−2	97	95	−2	96	97	+1

* Notation same as in Table I.

consciously or subconsciously, of an individual way of picturing the stimuli, sometimes also including the response, and which consists of a particular form of imagery; fifth, the use of this imagery until it becomes reflex; sixth, the appearance of rhythms in the ability to hold the image-changes in attention; and seventh the formation of associations between the giving of the stimulus and the response, after the use of the imagery becomes reflex.

RESULTS OF THE TEST SERIES.

The results of the test series for all observers are shown on Charts I to XIII inclusive by the broken lines which run directly across the chart. The results are also shown numerically in Tables I to III. The ability in each of these tests is expressed in the per cent of correct responses, as in the training records. The tests are discussed in the order in which they were taken.

Trained Observers.

Poetry. The gain in the tests for poetry was not very great in the case of any observer. Various methods were used. A large number read the stanza over first. Some divided it into parts of two lines each, some of four lines each, some into two parts. More than half of the observers speak of using imagery in remembering. Several say that they pictured the lines on the page in relation to each other. Several divided the stanza into parts according to the pictures it contained. Many memorized by these images in the stanza, and then combined the images into a whole picture. Two observers say that the training series may have helped in securing improvement. One says that the training may have helped by emphasizing imagery, and the other says that it may have helped in dividing the stanza into parts.

The Four Grays. The gain in the four grays is often greater than the gain in the training; it is usually as great, seldom less. In the first test the methods used were—to catch alternate groups, to divide groups into two groups of two figures each, and to remember each group. F. S., especially, noticed a tendency to image the groups. His first impulse was to remember the

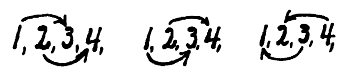
groups by a picture of the numbers of the grays or the grays themselves, but the time was too short to work out the picture for each group. Often observers tried two or three ways of remembering during the first test for the four grays. In the second test every trained observer but one says that the method developed in the training helped in securing a better record. Six of the trained observers say that they used the same imagery in the final test series that they used in the training series. There was more or less hesitancy in using the same imagery because of the difference in the stimuli of the training series and this test. The difficulty seems to depend upon the imagery. With G. C. F., whose imagery was visual-auditory for the tones of the training, there was great difficulty in using it with the grays. With D. S., whose imagery was visual, there was but a slight hesitancy in fitting the imagery into the responses for the grays. This was true also of H. C. E., A. R. F., M. M. M. and probably of E. M. C., for he says that the training helped him decidedly in the final tests, though he does not record a specific type of imagery. The same is true of M. L. F. S. says, in the introspection for the second test, that he remembered the groups by visual imagery with which he had no difficulty. He did not repeat the numbers as in the first test and as in the training series, but saw them in two groups of two in each group.

The Nine Tones. Four observers gained more in the nine tones than in the training series. Two made the greatest gain in the nine tones of any of the test experiments. The influence of the training therefore seems to be very strong. G. C. F. says that with him visual imagery is usually the strongest. Yet he made the greatest gain in the nine tones. His training imagery is auditory-visual, and he thinks that most of the gain in the second test for the nine tones is due to the influence of the training imagery. D. S. says that during the second test he was able to transfer his imagery system directly to this test by grouping the nine tones into fours. All the trained observers say that the training series helped in the second test. All except M. M. M. used the training imagery in the second test with the nine tones. M. M. M. divided the nine into numbers of three figures each, as 421, 343, 124. All had a different

way during the first test, and all found that the immediate succession of the second group of four tones after the first group led to confusion.

The Nine Grays. Three observers made a greater gain in the nine grays than in the training. One observer made his greatest gain in the nine grays. The introspections show the same characteristics regarding the influence of the training in the second test over the first. Two observers, G. C. F. and D. S., say that they find it easier to use their imagery with the tones than with the grays. But there is the same difficulty experienced in the immediate succession of the second group after the first, and the same change of method from the first to the second test. Under her introspection for this test M. L. descr bes her imagery:

"Toward the last of this series I thought of a new method of getting these by fours. It was by sort of picturing them with braces connecting them, with a top brace being the first one thus:



This would represent the combinations 1, 3, 2, 4, 2, 4, 1, 3, and 4, 2, 3, 1. This method helped me most when the grouping was something of the skipping order, and not 1, 2, 4, 3, where the numbers were right next to each other."

The Four Tones. One observer only, F. S., gained more in the four tones than in the training series, and no observer made the greatest gain in this test. As the name implies this test is most like the training series and therefore suggests that it should show the greatest gain if training is to influence tests similar to it. G. C. F. says that the failure to improve is due to the different method of response. Instead of responding by numbers the observers were instructed to respond by the syllables—Do, Me, Sol, Do-2. All observers say that there was a distinct tendency during the second test to use the methods and imagery of the training, but in the effort to make the syllable response, the tendencies developed in the training were broken up. Nearly all think that they could have made a better record if they could have responded by the use of numbers. The similarity to the

training series made the interference the more effective. M. M. M. says "the practice series was a hindrance here, because the numbers were so drilled into me that it was hard to change."

Geometrical Figures. Several observers think that the experience of the first test is sure to suggest methods for the second test. Motor imagery was strong with many observers for they moved the pencils over the paper while trying to retain the figures in memory between the trials of the test. The method most used was to remember the figures because of their similarity to letters of the alphabet. Some observers tried to remember the entire nine figures after a ten second exposure but most observers adopted the method of remembering two or three at each exposure. H. C. E. thinks that the training series helped here because he was able to group things together and think of two groups at once. M. M. M. knows of nothing in the training series that helped except that he was better able to concentrate attention on what he was doing. The other observers saw no connection between this test and the training series.

The Nine Numbers. F. S. made his greatest gain in this test. The common method here was to remember two or three of the first pairs, and to hold two or three of the last pairs because of their recency. No one recognized any way by which the training helped here.

Movement. The method employed by most observers is indicated by the introspection of D. S.: "The movements of the finger along the glass rod were always accompanied by an eye movement and a visual image of the distance traversed. It is evident that the estimate was made both by muscular and visual imagery." Not all observers recognized these factors but nearly all speak of visual and motor imagery.

Final Introspections. The opinion of the trained observers in regard to the factors that make for improvement and the relation of the training to the tests is shown by the following quotations from introspections written after the experiments were completed.

D. S.: "The following are the most important effects of the training series upon the test series:

1. The system of 'names.' The most important and effective factor.

2. Imagery was very prominent in the training and seemed to be more prominent in the second test than in the first.

3. Greater economy in mental effort and attention.

4. Development of ingenuity in devising methods. The method of 'names' was a very essential part of the improvement in the training series. Without it, I believe that I could not have reached the proficiency I did. In the last test, I felt that somehow I could better master the situation."

F. S.: "If I made any improvement in the last test, I think it was due to the following causes:

The idea that I was going to improve.

I found it easier to hold my attention on the work during the second test.

I felt a distinct sense of improvement in only one test, namely, the four grays. This was due to a change of method. I also have a feeling that I improved in the four tones. This may have been due to a general familiarity with the test. I was able to recall these tones with much less difficulty than the telephone tones. The gain in the four tones is due to the fact that I used the same method as in the training series. I had formed a certain habit of imagery which served me in this test."

E. M. C.: "There is a marked relation between the training series and the four grays, the nine tones, the nine grays, and the four tones, but apparently not much relation between the other experiments and the training series. Practically all the gain shown in the second test is due to the influence of the method used in the training series."

H. C. E.: "I think the thing that accounts for the improvement in the second test over the first is the system or method which was developed from the training series. In the last test series, I used the same method as in the training series, except that the system of imagery was changed slightly for the grays; that is, the grays appear in a vertical row just as for the tones, but instead of each being represented by a disk of different size they are now the same size and have the respective brightnesses of the four grays. I think it would make no difference what sort of tests I might be given where these four numbers were used; I could do as well as with the tones and grays."

Untrained Observers.

The methods used by the untrained observers will not be described unless they differ, in particular tests, from the methods used by the trained observers.

The Four Grays. Three of the four observers note a tendency to visualize the four grays. M. C. says, "The visual impressions seemed rather strong during the first test." In the second test, however, she abandoned the attempt to image. M. D. F. speaks of the use of an imagery he had adopted in some of the first tests. In the second test D. D. W. says, "I

tried to remember the grays as a row of figures; also to get a mental picture of them but the time was too short.''

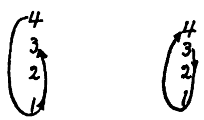
The Nine Tones. M. C. says that she saw the figures in a "group imagery" and retained them in that way. M. D. F. tried to repeat the numbers as tones, that is, high tones to low tones in each group, or low tones to high in each group of four. The four tones gave him an image of a board with four keys but he is not able to state the form of the keys or of the board, but tried to remember how the place or point suggested by each tone would skip around. In the introspection for the second test, he says:

"I can see fairly distinctly before me a key-board, and the tones go up and down. Four is at the top, and one is at the bottom, and I simply let the tone suggest the position, and when it comes time to respond, the image of the key-board returns. This test seems easier for me than the other experiments and also easier than the first test of this experiment, that is, it appears that the key-board helps me to remember."

With D. D. W. and J. W., a number of methods were tried during the first test. Both adopted the method of grouping by fours during the second test.

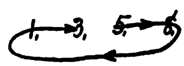
The Nine Grays. In the first test, M. C. tried to picture the numbers as they were given and also to note how often one of any kind was repeated. In the second test he tried to remember the nine grays in groups of fours, but this time they appeared not in imagery of figures, but as a picture of the dots. This aided materially in retaining the groups. J. W. says that he tried visualizing, but it was a complete failure because of lack of time to recall the picture in detail. M. D. F. says:

"Each color as it appeared suggested a number, and I had no trouble in classifying the colors. Nine, however, was too great a number for me to retain at once. Some groups I found much easier than others, for instance, 4, 1, 2, 3, and 3, 2, 1, 4, etc., appeared arranged in a vertical column, and attention skips from the four, which is always at the top, to the other numbers in the order in which they were given, somewhat as follows:"



M. D. F. used the same method in the second test. D. D. W. said that he tried to remember all the numbers of the group, but the nine seconds interval was too short, to form a distinct image of either the grays or the numbers they represent.

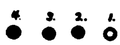
The Four Tones. M. C. thinks that the imagery which was used in the nine grays assisted here, but says that the different response interfered. In the first test J. W. tried to form an auditory image of the four tones but failed. He complains of an annoying fusion of the tones, a sort of harmony, which prevented him from remembering. With M. D. F. each note suggested a position on the piano, as 1, 3, 5, 8, and he remembered the order of the tones in that position. For instance, 5, 8, 1, 3, would be like this:



As long as he kept his mind concentrated on the above form he could remember, but it was very easy to allow the second group to confuse the one already received.

The final introspection for M. C. describes the form of imagery used and therefore it is quoted entire:

"I felt that I did better in the last end series than in the first, because I understood what I was supposed to do and was not confused. The only thing I know that helped me was the picture of the four dots, the dots appearing in a horizontal line,—



It seemed to me that if I could close my eyes during the test for the four and the nine grays, and not have the other group follow so closely, I could get them all correct. I used this imagery in my last tests for the four groups, but it seemed much easier with the grays than with the four or nine tones. This method did not help me in the other four tests. They seemed independent, and concentration in the first test did not help me in any of these four tests—geometrical figures, nine numbers, poetry, and movement. My greatest hindrance was that I often felt that I was not getting them right, and that confused me so that I could not concentrate my attention."

COMPARISON OF RESULTS.

Definition of Transference. It is first necessary for us to seek an understanding of what is meant by transference. We may mean by transference, the ability to use in one act the elements involved in another act. If we mean by transference that the training one receives in using a number of elements in one act is transferred to another act in which these elements do not occur, then the phrase "spread of training" would describe our meaning more accurately. In the sense of "spread of training," we can hardly say that there is "transference." A technical meaning of transference is answered only by the first definition. Both conceptions are involved in the present experiments.

If, as in the first definition, transference means simply the use of identical elements in different tasks, then the analysis of the conditions must be somewhat as follows: Let us suppose that any act is composed of the elements a, b, c, d, e; another act of the elements l, m, n, o, p, The two acts would be represented by two sets of elements, totally different, in which case there could be no such thing as transference. If, however, we examine an act composed of the elements a, b, c, d, e, and a second act composed of the elements e, f, g, h, i, where the element e appears in both acts, we would have the possibility of transference as far as the element "e" is concerned. If we had an act composed of the elements a, b, c, d, e, and another composed of the elements b, c, d, e, we would have a much stronger possibility of transference since all the elements except a in the first act are identical.

But, in the case of "spread of training" if we had an act composed of the elements a, b, c, d, e, and another act composed of the elements f, g, h, i, j, then the influence of training in the first act upon the elements composing the second act, will be measured by the amount of relationship between the elements of the two acts, that is, it will be measured by the amount of subconscious and actively conscious connection made between the elements by the observer. The application of the principle of "spread of training" can be made only to such cases where the elements concerned in the two acts are not identical, but related.

Of course, it were to beg the question if we used the phrase "natural connection," or the phrase "a natural relationship." The experimental difficulty involved is to determine which elements of experience are identical, which are related and which unrelated. The answer differs with individuals. Things related in one mind are not necessarily related in another mind. The relation between the things in experience depends, therefore, upon the relation which each person establishes in his own life—whether the frequent occurrence together of elements has built them into a purely automatic relation, or a connection is worked out by logical consciousness. At any stage of life an individual has a lot of relations which are automatic and another lot in which he has traced out a conscious relationship. We are often surprised by the discovery of a relation where we thought none existed. On the other hand we must guard against the conception that any mental components of a complex act enter into another complex act without being modified.

Relation of the Gain by the Trained to the Gain by the Untrained.

The following lists show the order of the tests arranged according to the greatest improvement for the trained and untrained observers:

Trained Observers.	*Untrained Observers.*
Four Grays	Nine Tones
Nine Tones	Nine Grays
Nine Grays	Geometrical Figures
Geometrical Figures	Four Grays
Four Tones	Poetry
Poetry	Nine Numbers
Nine Numbers	Movement
Movement	Four Tones

The rank of these tests, as shown by the greatest gain for each observer, is indicated in Table III. The four grays, with seven observers, show the greatest gain, and second greatest gain with two observers. The nine tones received first rank

with three observers, and second rank with five observers. The nine grays received first rank with two observers and second with two. The four tones received no first or second rank.

TABLE III.

Comparison of Results by Gain or Loss.

	G. C. F.	D. S.	F. S.	E. M. C.	H. C. E.	A. R. F.	M. M. M.	M. L.	M. C.	J. W.	M. B. F.	D. D. W.	Average Trained	Average Untrained	Difference between Trained and Untrained.
Training Series	32 (24)	25	−9	41	27	29	17	[9]					21		
Four Grays	22	*37*	*26*	*48*	56	*39*	*23*	*39*	2	7	*15*	−7	36	4	32
Nine Tones	*34*	35	1	15	*59*	8	5	15	5	9	14	*14*	22	11	10
Nine Grays	2	26	−2	12	56	32	2	21	*21*	*11*	0	8	19	10	9
Four Tones	−6	3	22	15	23	20	−16	19	2	−17	0	6	10	−2	12
Average	13	25	12	23	49	25	4	24	8	11	15	5	22	6	16
Difference between training gain and test of greatest gain	2	12	35	7	32	10	6	30							
Geom. Figs.	9	*11*	0	6	*35*	4	*18*	*22*	*7*	*20*	*5*	0	13	8	5
Nine Numbers	2	*11*	*4*	−2	3	4	9	7	−8	4	2	2	4	0	4
Movement	−1	−2	1	−1	−1	−1	−3	6	−2	1	−4	1	0	−1	−1
Poetry	*15*	−3	0	*19*	0	*10*		8	0	0	2	*5*	7	2	5
Average	6	4	1	6	10	5	6	11	0	6	1	2	6	3	3

All figures represent differences between first and last tests.
The record of the test which shows the greatest gain is printed in italics.
Minus sign means loss.

In Table III, the tests are ranked by similarity and by dissimilarity to training series. In the ranking for first and second places geometrical figures received first rank with six observers and second with three. The nine numbers received first rank with two observers, and poetry received first rank with four observers. Movement did not receive first, second or third rank with any observer.

The amount of gain of the trained over the untrained is shown by the amounts recorded in the last column of Table III. This column shows that for the four grays there was a difference between the results for the trained and the untrained of 32 per cent, or a gain nine times greater in the trained than in the untrained; for the nine tones, a difference of 10 per cent, or a gain twice as great for the trained as for the untrained; for the nine grays a difference of 9 per cent or a gain twice as great; for the four tones, a difference of 12 per cent; for geometrical figures, a difference of 5 per cent; for the nine numbers, a gain of 4 per cent; for the movement a difference of – 1 per cent; and for poetry a difference of 5 per cent.

The difference of the gain of the trained over the untrained in tests intentionally similar to the training series is 16 per cent. The corresponding difference between the trained and the untrained for tests intentionally dissimilar is 3 per cent. The gain of the trained in tests similar to the training is three and one-half times as great as their gain in tests dissimilar. The gain of the untrained in tests similar to the training is twice as great as their gain in tests dissimilar.

Let us now analyze, if possible, the influence of the training series upon each of these tests.

The four grays differed from the training series in but one factor, that is, grays instead of tones were used for stimuli. A reference to the table shows that the average gain for all observers in the four grays over the training series, is about 15 per cent. This is true of every observer except one.

The test showing the second greatest gain was the nine tones. Here the test differed from the training series in method, but was identical in content. In estimating the influence of the training upon test experiments in tones the first test may be regarded as the first practice series. This would tend to lift the recorded results for the first training above the first record for the tests in tones. This, in turn, would tend to increase the gain of the test over that of the training series. On the part of five of the observers there was a gain in the nine tones greater than the gain in training. The average shows a difference of 1 per cent in favor of the nine tones.

The nine grays show the third greatest gain. In the case of five observers the gain is greater in the nine grays than in the training. The difference between the averages of the two for all observers is greater by 2 per cent for the training than for the nine grays. The nine grays differ from the training series in both content and method.

The four tones differ from the training series in the use of pitch differences, instead of differences of intensity of the same tone, and in the method of response. The results show a gain in the training series greater than in the four tones, except for two observers. In reality there is a gain in the tones greater than in the training in the case of one observer only, for M. L. trained but two days. F. S., the other observer, made no gain in his training, but there was a gain of 22 per cent in the four tones. It seems that of those who gained in training and trained the full time, there was in no case a gain in the four tones equal to the gain in the training. In two cases there was a loss in the four tones. For all observers the average shows that the gain in the training was twice as great as in the tones. It is to be pointed out that this test is very similar to the training series. The different response required was planned because it was thought that it would be more familiar to the observers than the number response. As already mentioned, however, the introspections show that it was the different nature of the response that accounted for the failure to gain more in the four tones; several observers speak of this as a hindrance to correct responses.

Table III, shows also the difference between the gain in the training series and the gain in the test making the greatest gain. Every observer made a greater gain in these latter than in the training series.

The test showing the greatest gain for trained observers in the class of dissimilar tests was geometrical figures. In the case of two observers (including the observer trained for two days only) there was a greater gain in this test than in the training series for the same observers. This is the test among those unlike the training series in which the untrained observers make the greatest gain. This test also shows a greater gain in the

case of trained observers than does the test for the four tones. But for the other dissimilar tests there is so small an improvement that the trained observers gain on the average three and one-half times as much in the similar tests as in the dissimilar ones.

Table III indicates great variations in the amount of improvement made by different observers in the different tests. It might be expected, judging from the way the experiments were planned, that it would be easy to subtract the amount of improvement in the several tests from the improvement in the training series, and thus arrive at a direct estimate of the influence of the training upon that particular test. This, however, is not an easy matter. Individual differences and factors beyond those brought out in the numerical results enter to complicate the estimates. It is only possible to make such an analysis when the introspective evidence is sufficiently full and accurate to enumerate and define all the factors involved.

Three significant features have thus been noted in the above table; first, the difference between the improvement of the trained over the untrained; second, the difference of improvement in the tests similar to the training series in trained observers over their improvement in tests dissimilar; and third, the greater amount of improvement in the tests than in the training.

The Factors in Improvement and Transference.

The things most commonly mentioned by observers as contributing to improvement and transference have already been enumerated. It remains to point out some of the considerations bearing upon the interpretation of these factors.

Imagery. No suggestions were given observers regarding imagery. Indeed, it was not until the experiments were completed and work on results began that the uniformity of the testimony of observers concerning it was realized.

Eight of the twelve observers, all but two of the eight trained and two of the untrained, record a specific type of imagery. The two trained observers, F. S. and E. M. C., who did not do so, show by the language of their introspections that they

used imagery, but did not recognize a particular form. Of the untrained observers, two recorded the development of a complete imagery system, while with the other two there was a strong tendency to develop an imagery. As is shown by their introspections, in the case of observers who took only the test series, it is evident that the brevity of the tests together with the rapidity of change, would not permit the development of imagery with such facility as in the training series. Nevertheless two of the untrained observers, M. C., and M. D. F., developed a specific, well recognized imagery.

The fact that the stimulus of the training series was sound would lead us to expect auditory imagery. However, reference to the introspections already quoted will sufficiently indicate that there are three main types of imagery represented among the observers. G. C. F.'s may be called a visual-auditory type; that of D. S. and M. C., a purely visual type, and that of M. M. M., A. R. F., H. C. E., M. L., and M. D. F., a visual-motor type.

Everyone who does any act like this memorizing has a characteristic method. The evidence derived from these experiments indicates that the essential element in method is imagery.

Having once selected, consciously or unconsciously, an imagery, improvement seems to depend upon the fidelity of the observer to that imagery. Improvement depends also upon the fitness or adequacy of the imagery to do the thing for which it was adopted.

Whether each has an imagery for each separate act, whether each has a great many forms of imagery, corresponding perhaps to the customary things of life, or whether we have a few forms of imagery which we use for many different things, are interesting questions. If we do not have an imagery for each act, then the question of the use of imagery in different acts is just the one we are seeking to answer in regard to "transference" or "spread of training" by these experiments.

If an imagery is selected which is complicated, such as that of A. R. F., the observer is doing no other than selecting a complex method, which requires longer use to secure accuracy and speed. Or, if one selects an imagery which is not adequate to

the task as a whole, but serves for part only, such as an imagery for certain groups of sounds in the practice series, illustrated possibly by the imagery of D. S., then the observer must adopt a double or even a manifold system of imagery, and improvement in speed and accuracy would seem slower. Also, if one should change his type of imagery, it would lead to a lack of improvement or to fluctuation in improvement. This is a possible explanation of the failure of F. S. to improve.

For a short practice series, it would seem better to adopt a method or type of imagery as soon as possible, and, even though it is found to be cumbersome, remain faithful to it; for tasks long continued or to be oft repeated, the sooner one selects the best imagery, the better for the final outcome. Native ability finds its field in the readiness with which one selects an imagery adequate to secure the accuracy and speed demanded by a skillful performance of the task.

The prominence thus given to imagery as the essential characteristic of method has been pointed out before. Binet, in his "Psychology of Reasoning," insists upon imagery as the essential factor in all mental operations. Nearly every research in imagery since then has indicated something of the large place which imagery occupies in mental life. Coover and Angell have shown the value of the "careful elaboration of the plan of work, the actual working out of the method in the form of detailed introspections, and the searching and thorough analysis of results in experiments of this kind." Their research, however, not only fails to bring out the fact of an individual imagery, but even seems to seek to eliminate imagery altogether as a factor in improvement in training. Its presence, however, seems to be indicated in some of the introspections quoted.

"Am able to abstract from visual imagery of the apparatus entirely, and yet refer sounds to external stimuli. This seems to take the least effort, and is most satisfactory,"

They say—"The introspections indicate that the discrimination processes were accompanied by much imagery from other domains of sense, which in some cases determined the judgment. This imagery was largely kinesthetic and visual." "One reagent seemed to compare the intensities of bodily reactions to the sound stimuli themselves or to imagery called up by the stimuli, e. g., the 'flash of a bicycle lamp.'"

Still they say explicitly—"Many introspections * * * near the end of training were, 'No imagery.' "[1]

The relation of type of imagery to "transference" or "spread of training" is indicated in part by the results. In the case of G. C. F., the visual-auditory imagery used was that of a localization in space of the four tones. When in the test experiments, the four tones were changed to grays, there was a strong tendency to remember the grays in the same manner as the tones were remembered, because the stimulus rhythm and the method of response were the same. But the grays refused to take the position in space that had been customary in the case of the tones. An improvement was made between the tests for grays, but this tendency to use the practice imagery had to be overcome. The more thoroughly he was trained in the use of his imagery the less able was he to make a good record in the tests where he found a tendency to use it, but to which it seemed inapplicable. He gained about 20 per cent during his second practice period, but the results of the third test series shows not only no gain over the second test results but an actual loss of 8 per cent in the four grays, and of 6 per cent in the nine tones. These are, however, the tests in which he had made the greatest gain between the first and second tests. He gained but 3 per cent in the nine grays in the third test, and he made no gain in the four tones. His tendency to gain is shown by the gains of 11 per cent in poetry, 10 per cent in nine numbers, and 6 per cent in geometrical figures.

Nearly every observer, especially those who developed a clear imagery, was troubled with the same difficulty in the case of the four tones, for here the change in the response interfered with the use of the practice imagery.

On the other hand with D. S., H. C. E., A. R. F., M. M. M., M. L., M. C., and M. D. F., the type of imagery developed was as easily used with the grays as with the tones. These observers illustrate the benefits of making an imagery capable of being used in several acts thoroughly automatic as quickly as possible. With these observers, the longer

[1] Coover and Angell, *op. cit.*

they were trained, the easier it became to use the automatic imagery in the tests. So strong did this connection seem to H. C. E., that he said: "I think it would make no difference what sort of test I might be given where these four numbers were used; I could do equally well as with the tones and grays." The longer the observer was trained, therefore, the more non-essentials were cast aside, while the few essentials became habitual. When attention was long confined to the essentials, each element among them became welded into the imagery system.

Now, if a task differing in one essential only, from the trained one is given, the whole system feels the shock of the change in a vital part, until the adjustment is made. If the new task differed in two or three points, the shock is still greater. If the task was so different that the observer recognized no similarity, that is, if for him there was no way of applying his system of imagery, or if the imagery did not apply itself, then a new system of imagery was built up for the new task. It would seem, then, that the best time to make transfers of training in tasks which we recognize as dissimilar, is in the moments of beginning a new task, because the non-essentials which we use at first may be the essential ones in the second task. Thus, there may be advantages in learning several acts at about the same time.

Cases in which the amount of improvement in the test is greater than the amount of improvement in the training, are explained in part by the nature of the imagery used by the observers; the imagery used by the majority of observers was more readily applied to the tests than to the training. Such imagery as that of H. C. E., A. R. F., M. M. M., M. L., M. C., and M. D. F., supports such a view. The question of transference, then, becomes in very large part, a question of the nature of the imagery employed in the practiced task.

The significance of practice in the first test must be estimated here. Tables I and II show the difference between the first and second trial of each test, for both the first and second tests. It will be noticed that the gain between the first and second trials of the first test is often greater than the gain between the two tests. This is in accord with the well known fact that practice shows the greatest gain at the beginning of a training series.

The influence of one end test upon another is, therefore, the more serious in the "before" tests; and the effect of these tests upon the beginning of the training series may, in some cases, amount to more than the effect of a day of training in the training series.

The relation of improvement to one's ideas of improvement has often been raised as an experimental question in psychology. Many experiments have shown how often results differ from the feeling regarding improvement. It seems probable, from this series of experiments, that the feeling of improvement or the lack of it, is more or less closely connected with familiarity or lack of familiarity with imagery. All observers in this series of experiments were kept ignorant of results, but were asked to note in their introspections their own feelings regarding improvement. It often occurred that the feeling and the fact coincided. This seems to be more uniformly true in the case of those who developed a recognized form of imagery. It seems to be more often true in the case of those who did not recognize an imagery, and of those who had not yet recognized their imagery, that the fact did not correspond to the feeling.

The factor of attention and its control seems to be an important one in improvement and transference. In the opinion of observers it ranks next after imagery. Introspections at the beginning of the tests, and early in the training, show that observers recognize attention or the lack of its control as an important element in selecting the essentials from the non-essentials. Many speak of the rapid fluctuations of attention at this time. Observers who had a vivid imagery, speak of the fluctuations of attention in the use of the imagery; later in training, when the use of the imagery has become automatic, they say that co ntrol of attention seems to be the chief factor in rapid improvement, and the lack of it, the cause of error. Nearly every observer who seemed to approach the limit of his ability in training, testifies that the slightest fluctuation of attention produces a change in the results. In early training, therefore, attention seems to be drawn easily to the new conditions of work, i. e., to non-essentials. In improvement during practice, attention is more and more given to the central element concerned,

i. e., to the imagery which the observer uses. Toward the limits of training, attention may be permitted to run off on associations for the automatism of the imagery permits extra time between the stimulus and the response. When observers are making every effort to miss no stimulus or response, slight disturbances, such as slight changes in the stimulus, or noises from without, break the rhythm and produce rapid changes in attention. Practice curves of these observers, if plotted with regard to the grouping of the fours, where there is no change of method, are an excellent representation of the normal fluctuations of attention.

Association is another factor in training and transference. Most observers say that at the beginning of training and in the first test there is no time to form other associations than those among the elements concerned, but toward the close of training, nearly all speak of lapses of attention, due to associations with outside things formed in the interval between the stimulus and the response, or between the response and the stimulus. The relation between the training series and the test series may be called association, but it is better defined from the standpoint of imagery.

Automatisms have already been mentioned several times in connection with training and transference. It is inevitable that they should be formed in any process of training. The rapidity with which they are produced depends directly upon the fidelity of the observer to the imagery adopted, and upon the simplicity or complexity of the imagery to the observer. For example, H. C. E. adopted an imagery the first day of training and used it throughout training. His imagery was to him easy and readily used, and became automatic very quickly and thoroughly. A. R. F. did not recognize an imagery early and, when he did recognize it, it seemed to him complex and difficult of use. His imagery became automatic slowly, and before it became very thoroughly so, the training was over.

The relation of automatisms to the final tests is one of assistance, or of interference. The more automatic an act becomes, the less likely are its elements to be transferred to unlike elements. If it can be used, all goes smoothly. But if the task

is sufficiently different in content or method, for the observer, to awaken conscious efforts to use it in the new tasks, then automatism becomes a hindrance. In improvement in training, therefore, the more quickly automatisms may be cultivated the better. In transference, the cultivation of automatisms may be either a help or a hindrance according to the nature of the imagery of the observer.

GENERAL CONCLUSIONS.

The original research by Professor James (Psych., Vol. 1, p. 667), which served as the starting point for the investigations, contains this sentence, "All improvement of memory consists in the improvement of one's habitual methods of recording facts." Several experimenters have interpreted their facts for or against James' conclusions as seemed evident to them. The fact is, however, that many researches interpreted adversely are capable of interpretation to support his contention. A research which the writer carried on with Professor Gilbert, published in the University of Iowa Studies in Psychology, Vol. I, on "Practice in Reaction and Discrimination" left a distinct impression in the writer's mind that Professor James was wrong. The evidence of that same research seems now to be capable of an interpretation in support of Professor James as otherwise. Among the researches which have been interpreted as against James' conclusions are those of Judd (Psy. Rev., Vol. IX, pp. 27 to 39); several researches on cross-education, such as those of Scripture, Smith and Brown (Yale Studies Vol. II), Davis (Yale Studies, Vols. VI and VIII), Ebert and Meumann (Arch. f. d. ges. Psy., Bd. 4). The researches which take the ground apparently in support of Professor James are those of Thorndike and Woodworth (Psy. Rev., Vol. VIII), Bair (Psy. Rev., Mono. Sup. No. 19), Coover and Angell (Am. Jour. of Psy., XVIII, p. 328). A distinct effort to analyze the elements concerned in improvement in practice and in transference has characterized the later researches. As typical of this tendency, we may quote the researches of Thorndike and Woodworth, and of Coover and Angell. Thorndike and Woodworth say that after

practicing with rectangles 10 to 100 sq. cm., observers learn that one has a tendency to over-estimate all areas and consciously make a discount for this tendency, no matter how different other sizes or shapes of surfaces used in tests may be; also to look for the variations or the exceptional occurrences among the elements involved in training and in tests; third, learning to estimate in comparison with a mental standard, rather than an objective standard. This analysis of factors involved has a bearing only upon the tests carried on by these experimenters. They simply point out what seem to them to be the elements in their set of experiments.

Coover and Angell give a more translatable list of elements that seem to them concerned in improvement and transference:

> "We find, therefore, causes of the tranference of facility to be: (a) the formation of a habit of reacting directly to a stimulus without useless kinesthetic, acoustic, and motor accompaniments of recognition, which results in (b) an equitable distribution of attention to the various possible reactions so as to be about equally prepared for all; and (c) the consequent power of concentrating the attention throughout the whole series without distraction."

The elements that appear on the surface in our experiments are, while in the main in support of the analysis given by Thorndike and Woodworth, and Coover and Angell, contain elements both somewhat at variance with, and in addition to, those discovered in these researches. If, in the following from Coover and Angell: "Improvement seems to consist of divesting the essential process of the unessential factors, freeing judgments from illusions, to which the unnecessary and often fantastic imagery gives rise, and of obtaining a uniform state of attention which is less than a maximum," and "useless kinesthetic, acoustic and motor accompaniments of recognition," by "fantastic imagery" is meant such imagery as appears in our experiments or if it means such imagery as one of Coover and Angell's observers mentions, when he "seemed to compare the intensities of bodily reactions to the sound stimuli themselves or to imagery called up by the stimuli, e. g., the "flash of a bicycle lamp," then we must regard our results as distinctly divergent. Such imagery is an essential factor, if not the most essential factor in training and transference. With Coover and Angell's

general conclusion regarding the factors common in cases of training of dissimilar stimuli; i. e., "the habit of stripping the essential process of unnecessary and complicating accessories," we are in agreement.

In regard to the experiments of Thorndike and Woodworth, the difference between their conclusions and the conclusions of this series may be pointed out as follows: "After one gets mental standards of the areas, he judges more accurately, if he pays no attention whatever to objective standards." If Thorndike and Woodworth mean by this the same condition of imagery as developed in our experiments, which we imagine is possible, that is one point of agreement.

"Improvement in any single mental function need not improve the ability in functions commonly called by the same name. It may injure it." With this our conclusions also agree. Some definition, however, as Thorndike admits, needs to be made of the phrase "single mental function."

"Improvement in any single mental function rarely brings about equal improvement in any other function, no matter how similar, for the working of every mental function-group is conditioned by the nature of the data in each particular case." The results of our experiments do not support the statement contained in this sentence, especially in the first half of it. Improvement in many cases is absolutely greater in amount in the tests than in the training. The truth of the latter part of the quotation is verified in our experiments if the word "imagery" may be substituted for the word "data."

"The very slight amount of variation in the nature of the data necessary to affect the efficiency of a function-group makes it fair to infer that no change in the data, however slight, is without effect on the function." This our results corroborate.

"The loss in the efficiency of a function trained with certain data, as we pass to data more and more unlike the first, makes it fair to infer that there is always a point where the loss is complete, a point beyond which the influence of the training has not extended." Again our results corroborate.

"The rapidity of this loss, that is, its amount in the case of

data very similar to the data on which the function was trained, makes it fair to infer that this point is nearer than has been supposed. Again our results corroborate.

In the light of results here secured, we would change the following statement: "The general consideration of the cases of retention or of loss of practice effect seems to make it likely that spread of training occurs only where identical elements are concerned in the influencing and influenced function," to read—spread of practice occurs only where an imagery develops capable of being used by the individual observer in both training and test fields.

Our results do not corroborate the following statement from Coover and Angell, p. 339, as far as the freeing from any system is concerned:

> "At the beginning of training, they matched the color of the cards with the labels on the compartments; then to increase speed a system of mnemonics is employed, designed to form associations in the mind between a compartment and its color; this system then goes through a process of mutation,—becomes abbreviated, changed in part, supplemented, or is superseded by another; finally, through repetition, reactions to particular compartments become co-ordinated with their respective colors and are made directly—free from any 'system' except in rare cases."

The evidence from the introspections of all of our observers shows that there is no tendency to do away with the imagery or to free from the imagery system. Such cases as D. S., who had been trained by a long series of reactions practically identical with those in which he is trained here, and of G. C. F., who was trained for two months in the practice reactions used in this experiment, reactions which were selected for the intensity of application required in improvement and because of a possibility of reaching the limit of training for different observers within the practice period—such cases do not show any tendency to abandon the system. In this, therefore, our results do not agree with those of Coover and Angell.

With the statement of Professor James our results are in accord inasmuch as all the factors we have discovered have to do with methods.

There are two factors then, which we are seeking to analyze; first, to determine the factors that make for improvement; and

second, to determine the factors that make for spread of training or transference of training. If the problem were attacked from the standpoint of numerical results only, the analysis into elements would be most confusing.

SUMMARY OF CONCLUSIONS.

Some elements concerned in *improvement* and *transference* have been enumerated. Of these the central or most essential element is individual imagery.

Improvement seems to depend upon the consistent use of some form of imagery, whether it is the most advantageous form or not.

Imagery may be sub-consciously developed, but if it comes to be consciously recognized the improvement is more rapid. The rate of improvement seems to depend directly upon the conscious recognition of the imagery, and upon attention to its use.

A change of imagery during practice increases the rapidity of the improvement if a better form is adopted and adhered to. It may prevent improvement if a change of imagery is frequent, or if a less adequate form is adopted.

Individual differences are clearly shown in different types of imagery by the rapidity with which the imagery develops, and by the clearness or definiteness of the imagery.

The habit of guessing interferes with the formation of imagery and therefore, results in lack of improvement.

Transference may be divided into two kinds. It is either the use of identical elements in different tasks, or it is of the nature of "spread of training." The evidence of these experiments is in favor of the use of identical elements, or at least in favor of a limited spread of training. We are able to say that transference depends upon the nature of the imagery employed in practice, rather than upon any other factor. Whenever the training has become automatic and the difference between the training and the test consists of a few elements, these different elements serve as a hindrance only. We have then something of the nature of spread of training. If the difference is so very

slight that the elements are practically identical, as between the four tones of the training series and the four grays, there is little difference between the gain of the training and the test series. We have here something of the nature of transference, though transference as we have defined it, demands a complete identity between the elements of the acts. When the acts are made up of quite different elements, there is a distinct breaking up of the habit of responding, by the intrusion of the different elements, which raises the whole act into active consciousness so that the transfer of elements from one act to another act, other than the identical ones, is a conscious transference. It seems, therefore, that a conscious effort to use the elements of training in a different task, assists in making the transfer.

Factors that lead to improvement in the training do not necessarily lead to improvement in the tests; they may hinder it. The nature of the imagery, and the training in it seems to determine this. If, in the mind of the observer, the imagery is capable of adjustment to different tasks, it can be used in both improvement and transference, for the elements of the training act are thereby made the same as those of the test act. If it is adapted, in the mind of the observer, to the training task only, it may assist in improvement but it may interfere with transference.

Native ability appears to have abundant opportunity in the recognition of similarity or difference in the capability of the imagery for use in various tasks.[1]

[1]"Images, along with sensations, constitute the material of all intellectual operations: memory, reasoning, imagination, are acts which consist, in an ultimate analysis, of grouping and co-ordinating images, in apprehending the relations already formed between them, and in reuniting them into new relations." BINET, Psychology of Reasoning.

"Just as the body is a polypus of cells, the mind is a polypus of images." Taine, "On Intelligence."

THE EFFECT OF PRACTICE ON NORMAL ILLUSIONS

BY

C. E. SEASHORE, EDWARD A. CARTER, EVA CRANE FARNUM, AND RAYMOND W. SIES.

The following experiments are an outgrowth from experiments made by one of the writers in 1894 on the persistence of the size-weight illusion.[1] At the time that those experiments were made illusions of the kind were comparatively unknown and it so happened that the four observers experimented upon were all completely ignorant of the illusion. They had, however, been selected as the most intelligent and cautious observers among the advanced students. The experiments ran through twenty days, a half hour each day, with each observer. All exhibited a strong normal illusion, somewhat larger than that found as the average for ten other observers of the same type. And all came out alike in showing the surprising conclusion that the twenty days of practice revealed no tendency to decrease the illusion. At the conclusion of these experiments the results and the significance of the illusion were explained in detail to each observer and another test was made to determine the effect of knowledge of the illusion. Again the results were alike for all the observers showing that the knowledge of the illusion immediately decreased it by nearly one-half of its original force.

The measurements on the persistence of the illusion of the vertical made by Dr. Williams in 1901 also belong to this series of studies.[2] She had three observers and trained them for ten days each making one hundred trials each day for each observer.

[1] Seashore, "Measurements of Illusions and Hallucinations in Normal Life," Yale Studies in Psychology, III, 5–9.

[2] Williams, "Normal Illusions in Representative Geometrical Forms," Univ. of Iowa, Studies in Psychology, III, 108–116.

The first of these observers had no knowledge of the illusion, the second had partial knowledge, and the third (C. E. S. of the present series) had full knowledge of it. The first two observers exhibited an abnormally large illusion, both averaging 21%; the third averaged 6%. The following conclusions were drawn:

> "The illusion fluctuates in strength from day to day, especially for the observers who are aware of its existence.
>
> The practice gained in one thousand trials does not decrease the force of the illusion of the vertical for the line: this is equally true for those observers who know of the illusion and those who do not know of it.
>
> For one observer, who has had extensive experience in the observation of this illusion for years, the illusion still has a normal force."

The results of the investigation first mentioned were remarkable in that they demonstrated the normal persistence of the illusion so long as the observer has no knowledge of it. The second investigation resulted in another surprise in that the illusion persisted also in those observers who had knowledge of it. In the meantime Judd[1] reported experiments on the Mueller-Lyer illusion showing that the illusion disappeared with practice, and without leaving any conscious trace of the process of correction. This became a further stimulus to a search into the conditions which determine the effect of practice, especially with reference to different types of illusion, different degrees of knowledge, different capacities in critical attitudes, duration of practice, and the consciousness of gain.

To contribute toward the solution of such problems the following four studies have been undertaken in coöperation, each devoted to one type of illusion. The first named writer is responsible for the general plan and supervision of the experiments and has written this article as a synopsis of the four independent reports written by the respective experimenters.

[1] Judd, "Practice and its Effects upon the Perception of Illusions," Psychological Review, IX, 27–39.

I. THE ILLUSIONS IN THE LENGTH OF A CYLINDER.

MEASUREMENTS BY EDWARD A. CARTER.

A cylinder looks to be longer than it really is. This overestimation has been analyzed by Williams[1] into several constituent illusions. Thus, the dimensions of a surface are overestimated when compared with a line; the surface of a solid is overestimated when compared with a plane surface, and, there is some peculiarity about the cylinder which leads to a further overestimation of its length. These three errors Williams has called respectively the area illusion, the volume illusion, and the illusion of cylinder length. When a cylinder is placed in a vertical position, the illusion of the vertical also enters.

Four observers went through similar training series on this illusion in its gross form, without any attempt to isolate the constituent elements.

As illusion objects we used three black metal cylinders, each 114 mm. in diameter and, respectively, 109 mm., 114 mm., and 119 mm. in length. The object in using more than one cylinder was to prevent the forming of any absolute standard of the length. The practice consisted in repeated measurements which were made in terms of a straight line by the method of production with the apparatus described by Williams.

This apparatus consisted essentially of a frame one meter square placed in an erect position and covered with manilla cardboard near the center of which a 2 mm. watch spring protruded through a slit and lay flush against the surface. The length of the exposed part of this spring was regulated by cords in the hands of the observer and a permanent millimeter scale on the back enabled the experimenter to record each setting. A similar frame having an inconspicuous wire support for the cylinder near its center was placed by the side of this. The frames were placed edge to edge and so turned that their centers would be on a level with the eyes of the observer and at right angles to the line of regard of the observer when seated at a distance of one meter.

[1] Williams, M. C., "Normal Illusions in Representative Geometrical Forms," Univ. of Iowa, Studies in Psychology, III, 38–139.

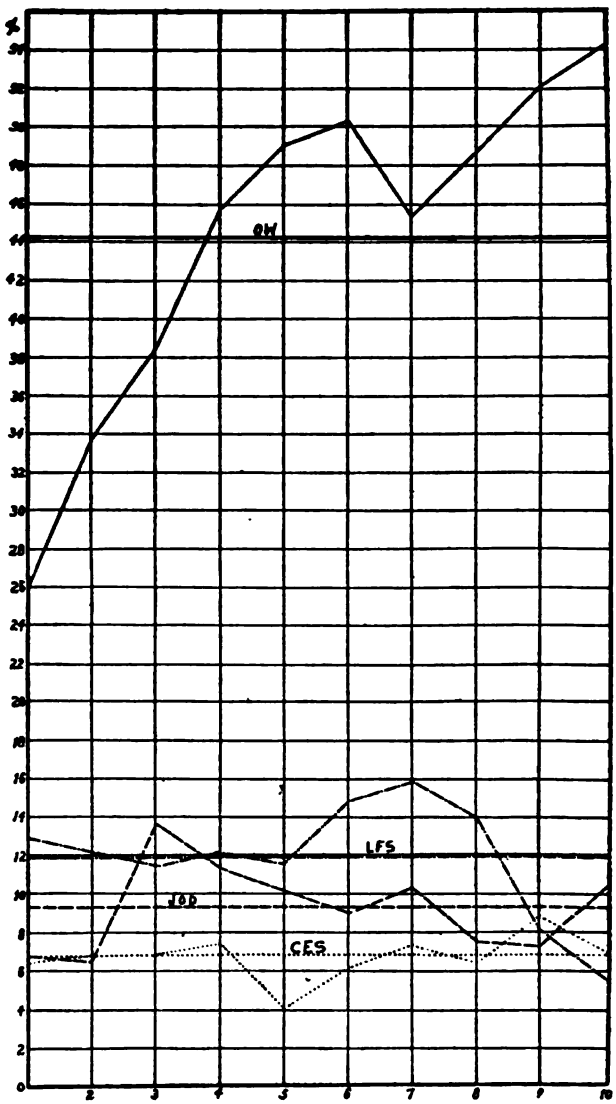


FIG. 1. Horizontal Position.

Observations were made on both the vertical and the horizontal positions of the cylinders. The vertical length was measured in terms of a vertical line and the horizontal length in terms of a horizontal line. Repeating this on the three cylinders made six independent sets of observations. Ten settings were made for each of

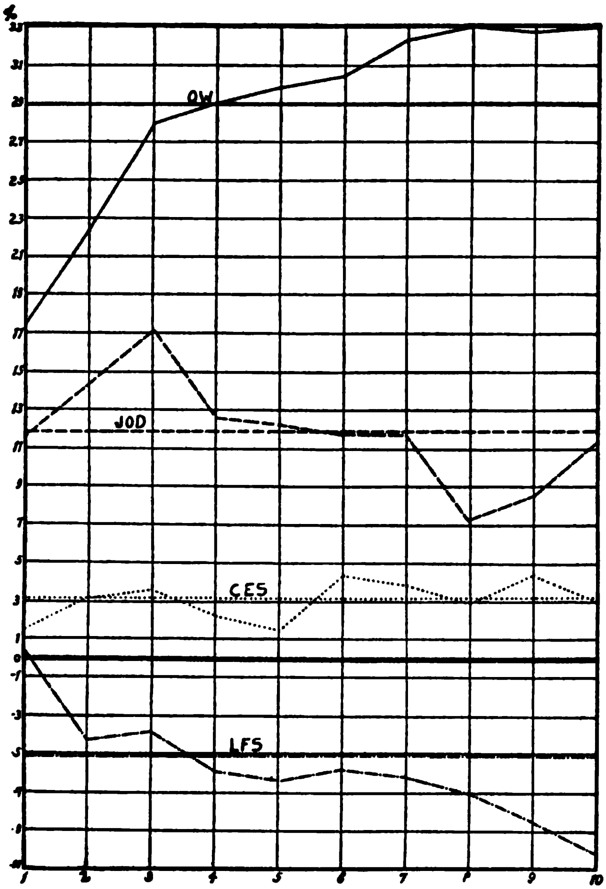


FIG. 2. Vertical Position.

these in each training period, which usually lasted an hour. The trials were made in the double fatigue order. The observers were allowed to look back and forth from the cylinder to the line as often as they wished, but were required to turn the head and not merely the eyes. All conditions of the experiments were

kept as nearly constant as possible. The experiments were made at the same time of the day and, as nearly as possible, on ten consecutive days. The observers were kept in complete ignorance about their results throughout the entire series. The task before the observer was quite simple—merely to represent, with the spring, a line which he was satisfied looked equal to the length of the cylinder. Unavoidable changes of attitude and development of theories were noted in introspective notes. The experiments were made during the winter, 1903–04.

The results of the experiments upon the four observers are contained in Tables I to IV, the average amount of the illusion and the mean variation for each day being given in percentages for the ten successive days.

Since the 109 mm. and the 119 mm. cylinders were used only for 'confusion' and the results for these differ in no essential way from the 114 mm. cylinder, the results of the three are combined and thrown into practice curves, Fig. 1 and 2. The percentage of illusion, figured on 114 mm. as a base, is indicated on the ordinates and the periods of practice on the abscissae. The average illusion for the ten days is indicated for each curve by a horizontal line of the same type as the curve.

A glance at the curves reveals the fact that the observers react to the practice and conditions of training in different ways. C. E. S. has a small illusion which remains approximately constant. L. F. S. unconsciously reacts to his knowledge of the illusion and reverses it. J. O. D. has an average illusion and gives no certain evidence of lowering it by practice. O. W. starts out with a strong illusion which rapidly increases throughout the whole training. We must consider each of these in some detail.

C. E. S. had full knowledge of the nature and force of the illusion, as well as of all the conditions of the experiment. He had had varied and extensive experience in the analysis and measurement of all the illusions involved. He expected the series to start with an illusion of about 10% for the vertical position, but did not have so definite expectation in regard to the horizontal position. Steadily avoiding to 'think it out,'

TABLE I. (C. E. S.).

A. Horizontal position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	5.5	1.8	5.3	1.8	8.4	2.3	6.4
2	7.3	1.8	6.1	1.7	6.7	1.7	6.7
3	7.3	1.8	6.1	1.8	6.7	2.3	6.7
4	7.3	1.8	7.0	0.9	7.6	2.3	7.3
5	3.7	0.9	5.3	2.7	3.7	2.3	4.2
6	6.4	1.8	4.4	1.8	7.6	1.6	6.1
7	7.3	1.8	7.0	0.9	7.6	1.7	7.3
8	5.5	2.6	6.1	1.7	7.6	2.3	6.4
9	9.2	1.8	7.9	1.8	9.2	1.6	8.8
10	7.3	1.8	5.3	1.8	8.4	1.7	7.0
Ave.	6.7	1.8	6.1	1.7	7.2	2.0	6.7

B. Vertical position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	2.7	1.8	1.8	1.8	0.0	2.3	1.5
2	3.7	2.7	3.5	1.7	2.3	1.6	3.2
3	2.8	1.8	4.4	2.6	3.4	2.3	3.5
4	1.8	1.8	2.6	1.8	2.3	1.7	2.1
5	1.8	1.8	2.6	1.7	0.0	1.6	1.5
6	3.7	1.8	5.3	2.6	4.1	1.7	4.4
7	4.6	1.8	3.5	1.8	3.4	1.6	3.8
8	3.7	1.7	2.6	2.6	2.3	1.6	2.9
9	3.7	2.7	5.3	1.7	4.1	0.8	4.4
10	3.7	2.7	2.6	1.8	3.4	1.6	3.2
Ave.	3.2	2.1	3.4	2.0	2.5	1.7	3.0

TABLE II. (L. F. S.).

A. Horizontal position.

Day	109 % Il.	109 % m.v.	114 % Il.	114 % m.v.	119 % Il.	119 % m.v.	Ave. % Il.
1	13.8	2.7	12.3	5.3	12.6	2.3	12.9
2	12.8	2.7	14.0	1.7	10.0	1.6	12.3
3	13.8	0.9	11.4	3.5	9.2	1.6	11.1
4	14.7	2.7	12.3	2.6	9.2	2.3	12.2
5	12.8	1.8	11.4	1.7	10.9	2.3	11.7
6	15.6	1.8	14.9	1.7	13.4	1.6	14.6
7	17.4	1.8	15.8	1.7	14.3	1.6	15.8
8	12.8	1.8	13.2	2.6	16.0	0.8	14.0
9	9.2	1.8	7.0	0.9	7.6	1.6	7.9
10	5.5	2.6	6.1	0.8	5.0	1.6	5.5
Ave.	12.8	2.1	11.8	2.2	10.8	1.7	11.8

B. Vertical position.

Day	109 % Il.	109 % m.v.	114 % Il.	114 % m.v.	119 % Il.	119 % m.v.	Ave. % Il.
1	3.7	0.9	− 0.9	1.7	− 1.6	1.6	0.4
2	− 3.7	1.8	− 2.6	1.7	− 6.7	0.8	− 4.3
3	− 2.7	1.8	− 3.5	1.7	− 5.0	2.3	− 3.7
4	− 3.7	3.7	− 6.1	1.8	− 7.6	1.6	− 5.8
5	− 3.7	1.8	− 7.9	1.7	− 7.6	3.4	− 7.1
6	− 4.6	2.7	− 6.1	1.8	− 6.7	1.6	− 5.8
7	− 4.6	1.8	− 6.1	1.8	− 7.6	2.3	− 6.1
8	− 5.5	1.8	− 7.0	1.7	− 8.4	1.6	− 7.0
9	− 9.2	1.8	− 8.8	1.8	− 9.2	1.7	− 9.1
10	−11.0	1.8	− 8.8	1.7	−10.9	1.6	−10.2
Ave.	− 4.5	2.0	− 5.8	1.7	− 7.1	1.8	− 5.8

Table III. (J. O. D.).

A. Horizontal position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	7.8	0.9	6.1	2.6	6.7	1.6	6.7
2	7.3	2.7	6.1	3.5	5.9	2.3	6.4
3	12.8	2.7	14.9	2.6	13.4	2.3	13.7
4	12.8	2.7	10.5	1.7	10.9	1.6	11.4
5	9.2	1.8	12.3	1.7	9.2	2.3	10.2
6	6.4	2.7	10.5	1.7	10.0	2.3	9.0
7	11.0	2.7	11.4	2.6	8.4	1.6	10.2
8	7.3	2.7	7.9	1.7	7.6	2.3	7.6
9	5.5	1.8	7.9	2.6	8.4	2.3	7.3
10	6.4	1.8	11.4	2.6	13.5	1.6	10.4
Ave.	8.6		9.9		9.4		9.3

B. Vertical position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	11.9	1.8	12.3	1.7	10.9	1.6	11.7
2	16.5	1.8	14.9	2.6	12.6	2.3	14.7
3	18.3	1.8	17.5	1.7	16.0	1.6	17.3
4	13.8	1.8	12.3	2.6	11.8	2.3	12.6
5	11.0	1.8	13.2	0.9	12.6	3.4	12.0
6	11.0	1.8	12.3	1.7	11.8	2.3	11.7
7	11.9	1.8	12.3	1.7	10.9	1.6	11.7
8	7.3	3.7	6.1	4.4	8.4	3.4	7.1
9	7.3	2.7	9.6	3.5	8.4	2.3	8.4
10	11.9	1.8	11.9	1.8	10.9	2.3	11.6
Ave.	12.1	2.1	12.2	2.2	11.4	2.3	11.9

TABLE IV. (O.W.).

A. Horizontal position.

Day	*109* % Il.	*109* % m.v.	*114* % Il.	*114* % m.v.	*119* % Il.	*119* % m.v.	*Ave.* % Il.
1	22.9	5.5	27.2	4.4	26.9	3.4	25.7
2	32.1	3.7	36.0	3.5	32.8	2.3	33.6
3	40.4	3.4	38.6	3.5	36.1	3.4	38.4
4	46.8	3.8	44.7	2.6	45.4	4.1	45.6
5	49.5	6.4	49.1	3.5	47.9	4.1	48.8
6	52.3	3.7	51.7	2.6	46.2	4.1	50.1
7	50.0	3.7	39.5	3.5	46.2	2.3	45.2
8	51.4	2.7	47.4	3.5	47.1	5.0	48.6
9	53.2	3.7	54.4	3.5	47.9	3.4	51.8
10	56.9	3.7	53.5	2.6	51.3	2.3	53.9
Ave.	45.6	4.0	44.2	3.3	42.8	4.0	44.2

B. Vertical position.

Day	*109* % Il.	*109* % m.v.	*114* % Il.	*114* % m.v.	*119* % Il.	*119* % m.v.	*Ave.* % Il.
1	17.4	3.7	19.2	3.4	16.0	3.4	17.5
2	27.5	3.7	21.9	2.6	18.5	3.4	22.6
3	26.6	3.7	24.6	4.4	32.8	5.0	28.0
4	29.4	5.6	28.9	7.0	28.6	6.8	29.0
5	33.9	4.6	24.6	5.3	30.2	2.3	29.6
6	32.1	3.7	32.4	4.4	26.9	2.8	30.5
7	35.8	2.8	30.5	3.6	31.1	2.3	32.5
8	35.8	3.7	33.3	2.6	30.3	3.4	33.1
9	32.1	3.7	36.0	3.5	30.3	2.3	32.8
10	33.9	4.6	34.3	4.4	31.1	3.4	33.1
Ave.	30.5	4.0	28.6	4.1	27.6	3.7	28.9

he continued throughout to feel that the illusion for the horizontal position would be less than for the vertical.

The average illusion in the horizontal position amounts to 6.7%; and in the vertical to 3%. There is no evidence of any constant tendency to change the force of the illusion with practice, and there is no appreciable progressive change in the mean variation. Both the per cent of illusion and the per cent of mean variation come out approximately equal for the three cylinders.

Among the introspective observations of C. E. S. the following are noteworthy:

"There is a much greater temptation to correct for the illusion of the vertical in the line than in the cylinder. It is more difficult to disregard a relatively simple illusion which readily intrudes in consciousness than one which is more complex and therefore becomes focal in consciousness only with effort. This may account for the difference in the force of the illusion in the vertical and the horizontal positions.

It takes greater effort to see the length of a cylinder than the length of a line. Looking beyond the near side of the cylinder gives distinct sensations of effort through the eye muscles. This peculiarity of effort is particularly noticeable in a visual comparison of the height of a cube or a square with the height of the cylinder. This suggests an explanation of the illusion of cylinder length which Williams isolated and determined quantitatively but was unable to explain on the data in hand at that time.[1]

There seem to be amœboid movements in the line; as one tries to size it up, there seems to be a creeping lengthening and shortening. This is traceable to change in the mode of regard; when one tries to get the line as a whole, there is a tendency to converge the eyes for a point beyond the plane of the line and, consequently, the line seems shorter—the retinal image being of a given size but the line being judged to lie farther away than it really is. This suggests an explanation for the illusion of filled space in a line which is simply bisected: the bisection favors the effort to regard the line as a whole and, therefore, the tendency to converge for a point behind the plane of the line.

It was soon noticed that these creeping, amœboid movements were invariably referred to one end of the line, the movable one. (Bear in mind that the line was represented by a spring which protruded through the background and that its length was changed by shoving the spring in or out.) The method employed afforded especially favorable conditions for the apperception of this effect. Ordinarily we are so sure that a line does not change in length while we look at it that we inhibit the actual sensory process. Here the line was being adjusted so frequently that I became particularly appreciative of movements, both real and apparent.

[1] Williams, op. cit.

There was a tendency to remember the length of the horizontal line and to be influenced by this absolute standard in the perception of the vertical. (For this observer, who was aware of the differences of the cylinders, this tendency may have contributed toward the difference in the illusion for the figures in the horizontal and the vertical positions.)

There is a tendency to turn the eyes instead of turning the head. This may account for the error often found in comparing the length of two objects some considerable distance apart. The one to which the eyes are turned without turning the head tends to be overestimated."

In order to determine whether the illusion is likely to be less when one feels particularly satisfied with a measurement, C. E. S. followed the plan of recording cases in which he felt especially sure and satisfied that the adjustment of the line was right. There are in all sixty-three such cases distributed as follows: Twenty-seven give a smaller illusion than the average for the day by an average of 2.0%; twenty-five give a larger illusion than the average for the day by 1.6%; eleven cases coincide with the average for the day. Taking the three groups together, they give an average mean variation of 1.5%. The average mean variation for the series is 1.9%; the 'sure' cases are, therefore, somewhat more reliable; and the number of these cases above the average illusion is about equal to the number below but those below are 0.4% farther away from the average than those above. Hence there is no prominent tendency except in the direction of a smaller mean variation.

L. F. S. was a graduate student with a keen and brilliant mind. He was familiar with the illusion of the vertical. He also knew that the cylinder looked longer than it really was, and this knowledge, though undifferentiated, was of a sort of bogey order because he had seen the illusion demonstrated in some extreme forms. He was not informed about the dimensions of the cylinders or the order in which they were presented.

His average illusion in the horizontal position is 11.8%. While there is a complete wave in the latter part of the curve, it cannot be said that there is any constant progressive change with the practice. His recognition of the area and the volume illusions on the ninth day, however, may account for the lowering of the curves on those days.

The average illusion for the vertical position is negative, aver-

aging – 5.8 %, and in this there is a uniform increase throughout the practice. He starts with no illusion and ends with a minus record of – 10.2 %. The mean variations remain approximately constant for both positions throughout the ten days.

In the introspective account written after the tenth day, but before knowing the results, the following items are particularly relevant:

"Feel that I have not improved during the course of the observations. If there is any change, so far as I know, it must be due to practice."

"Knew of the illusion of the vertical though I did not attempt to use this knowledge. The same is true of what I thought to be a 14% illusion of cylinder (length)."

"Of the three cylinders, I think that the longest two are equal in length and that, in one of these, the length is equal to the diameter and, in the other, the length is greater than the diameter. The length and the diameter of the short cylinder are also equal."

"I think that the illusion would be more pronounced in the vertical cylinder than in the horizontal. My records for the horizontal cylinders are more accurate than for the vertical. Occasionally I was aware of a tendency to make my judgment of the length of the vertical line by comparing it with what I remembered to be the length of the horizontal line for the corresponding cylinder. If then I would make the vertical seem equal to the horizontal, I would make it too short, on account of the illusion of the vertical."

"Given a cylinder in the vertical position and a line in the vertical position, I think there would be a tendency to make the line too short. I became more aware of this tendency during the latter part of my observations."

On the ninth day he records that, instead of regarding the whole cylinders, he imagined a line drawn from end to end on the near surface of the cylinder. This is important because it is a condition which would tend to eliminate the illusions in the cylinder. (See notes by C. E. S. above.) The same day he records that he had not before thought distinctly of the effect of the area and the volume illusions. These two changes in attitude easily account for the downward turn in the curve in Fig. 1 on the ninth and the tenth days.

The explanation, then, of the results for the training for L. F. S. are essentially these. For the horizontal position the illusion is normal to a person 'with knowledge;' the lessening of the illusion on the last two days is accounted for by the two changes in attitudes, named above. This lowering is, however, not

greater than the immediately preceding rise, for which we have no explanation.

The reaction in the vertical position is partly accounted for by (1) the tendency to correct for the illusion of the vertical in simple and not in complex forms, as noted in the case of the first observer; (2) the bogey character of the gross illusion of length in the cylinder in the vertical position. (This observer did not know the force of the illusion for the horizontal position, and his estimate of 14% for the vertical position was a conservative estimate with reference to his own critical and discriminative attitude which excluded such force of the illusion as may be due to indiscriminate estimates); (3) the fact that he thought that the illusion would be more pronounced in the vertical than in the horizontal cylinder; and, (4) the fact that he considered the record for the horizontal cylinder most accurate, there being a feeling of unrest with reference to the vertical. The extreme negative results for the last two days have been accounted for above.

J. O. D., a liberal arts junior, was a distinguished athlete, slow in all his movements but a keen observer. He had heard an elementary lecture on the type of illusions involved and knew that the experiment involved these, but he did not have specific data clear in mind as a basis for correction although he was able to name some of the illusions. He had had no practice on this or any other illusion. The general effect of his information was to put him particularly on guard against all possible sources of inaccuracy. He accepted, without much questioning, the plain directions to make every possible effort to improve in accuracy.

The average illusion for the horizontal position is 9.3%; and, for the vertical, 11.9%. In neither case is there any constant tendency toward progressive change with the practice.

On the third day the observer recorded that he had been making the lines too long this day and attributed it to excessive fatigue. This explanation can, however, not be accepted without taking into consideration that he reported the same kind of fatigue and dullness on the fifth and the ninth days, and the illusion was almost average on the fifth day and below average

on the ninth. On the fourth day he reported a tendency to allow for the volume illusion 'by making the line a little longer.' This allowance was, of course, in the wrong direction; its effect is seen especially in the curve for the horizontal position.

He also noted the change corresponding to the fall in the curve on the eighth day and rightly attributed it to a scheme of imagining a plain line on the face of the cylinder, just as L. F. S. had done on the ninth day.

This observer noticed the "amoeboid" movements of the line on the first day.

O. W., a bright and painstaking liberal arts freshman, was a naïve and unprejudiced observer. He had not studied psychology and knew practically nothing about illusions. Special care was taken to keep him from getting any suspicion of the existence of illusions during the experiment. As a good student, he took the instructions in good faith and worked most diligently in the daily effort to cultivate accuracy in the use of his eyes.

His average illusion for the horizontal position is 44.2%; and, for the vertical, 28.9%. In both there is an unmistakable progressive increase in the illusion.

The observer being untrained, and the experiment being conducted with the effort to maintain a naïve state of mind, no introspective account was obtained. When he was shown the results at the conclusion of the series he was shocked, simply surrendered in a sort of despair and had no explanation to offer.

Our experiments resulted in no simple law of the effect of practice, but they enrich our insight into the actual complexity of the process. The four observers each represent an individual type of practice effect.

In the eight curves there are three cases of marked progressive change—two of increase in the illusion, and one of increase in the over-correction for it. The other five curves indicate no progressive change. To one who knows the observers and the conditions under which they worked these results seem 'strangely' natural. The first three observers had knowledge of the illusion and this probably reduced its force by as much as one-half. C. E. S. had gone through so much general training in illusions as to be free from disturbing motives which are due to

lack of a true point of view or lack of a discriminative attitude. J. O. D. was objective minded and naturally maintained the sensory-discriminative attitude in which the more rigid motives for the illusion gained uniform expression. L. F. S., who had not had sufficient training to guard him against the danger of being influenced by a partial knowledge of the situation, gave way to his analytical tendencies and made semi-conscious corrections, progressively increasing, in the vertical position. O. W. started with the strong illusion characteristic for hose who have no knowledge of it and, finding the task increasingly perplexing, probably changed mode of regard, etc., but for the worse and the illusion gradually increased.

If the drop in the curve for L. F. S. had taken place in the presence of the normal illusion there would have been danger of interpreting it as a clear case of gain through practice, but here it is clearly shown to be merely an expression of prejudice. This case is therefore particularly noteworthy.

The rise of the curve for O. W. is also noteworthy because it takes place for a person who already has a very strong illusion.

There is no distinct progressive tendency to increase or decrease the force of the illusion during the individual sittings, except in the three curves which show the progressive change for the whole series. There are many temporary fluctuations in the curves which may be accounted for by changes in method, etc., which one cannot fully preclude, but we have here discussed these experiments only from the point of view of progressive change.

II. THE T-ILLUSION.

Measurements by Eva Crane Farnum.

If two straight lines are joined in the shape of a plain capital T the one which is bisected seems to be shorter than the other. This is true though in different degrees, in all positions of the figure, when the illusion of the vertical has been eliminated.

This illusion was selected as representing a type which is probably due to lack of discriminative observation. It is usually very strong for one who is not aware of it. It was thought that mere practice, without information, would lead quickly to

discriminative apperception which would eradicate the illusion.

The illusion has never been fully explained. An analysis of the figure reveals several motives. First, as the two lines are at right angles, the illusion of the vertical enters. When the bisecting line is in the vertical position, the illusion of the vertical coöperates with the T-illusion; but, when the bisecting line is in the horizontal position, the illusion of the vertical counteracts the T-illusion. This motive may be fairly eliminated by studying the figure in both vertical and horizontal positions.

The T-illusion proper may be reduced to several component factors, and it is not unlikely that different motives operate in different methods of judging.

(1) The single division of a straight line is one constant factor. It is well known that, while filled space is usually overestimated, there is a paradoxical exception[1] in the fact that a single interruption, such as a bisection, leads to underestimation. The cause of this is a small but rigid motive about which there are several well-known theories. (2) Contrast enters in that a short line is compared with a long line when, as is often the case, half of the whole line is compared with one end of the bisected line. This has been demonstrated in three forms, namely: (a) the double square, (b) the two sides of the double square in the shape of L, and (c) two plain horizontal lines, one twice as long as the other.[2] (3) Confusion of whole and half of the bisected line, impossible though it may seem, is probably the main motive for the illusion when it appears very strong, as in children or adults who lack power of keen discrimination. There is a sort of subconscious tendency to select a variable line that is shorter than the whole bisected line because there is a vague craving for comparison with the one-half of it.

Three observers engaged in a practice series taking a minimum of a half hour a day for twenty days in the most intensive form of practice under the given conditions. The days were

[1] Wundt, "Geometrische optische Täuschungen," 82.

[2] Seashore and Williams, "An Illusion of Length," Psychological Review, VII, 592.

nearly consecutive. Two of the observers were selected with the hope of obtaining naïve results and training without theory or knowledge of the records during the practice series. The third man was a psychologist familiar with the illusion.

The measurements were made by the method of selection. The T-figure was drawn on a series of fifteen white cards, 36 cm. square, in black ink with lines five-eighths of a millimeter in width. The bisected line was equal in all the figures, 114 mm. The other line was varied by five-millimeter steps from 79 mm. to 149 mm. The cards were so frequently changed as to prevent identification. These experiments were made in the fall of 1905.

The cards were exposed one at a time against a neutral background at a distance of one meter from the eye and at right angles to the line of regard. The observer was required to state at each exposure whether the undivided line seemed longer, equal to, or shorter than the bisected line. The experimenter followed a definite plan in presenting the cards, as follows: The experiment might begin with any card, but after that, the observer's reply determined which card should be presented next and the plan was so arranged that the cards were selected in a continuous zigzag crossing the region of equality. The procedure may be represented in the following scheme, in which the numbers denote the length of the variable lines and the letters denote the three respective judgments Longer, Shorter and Equal:

79	84	89	94	99	104	109	114	119	124	129	134	139	144	146
			S	S	E	E	L	L		106.5				
		S	S	E	E	E	L			104.0				

This method is very effective and enables one to work economically and without fear of bias or knowledge of the results.

Four series were made by placing the figures successively in each of the four cardinal directions. In speaking of the directions, we shall refer to the direction of the variable line as well as to the number of the positions, thus: (1) ⊥ vertical-up; (2) ⊣ horizontal-left; (3) T vertical-down; and (4) ⊢ horizontal-right.)

The practice was distributed equally among the four series and, on the average, about 130 judgments were made in each sitting.

Among the methods, as learned from the introspections, the following three were frequently used and will be referred to by numbers, as follows: (1) The judgment was based, upon the general impression of the figure as a whole without any separation into parts. (2) The standard or bisected line was superposed upon the variable or undivided line. (3) One end of the standard, or bisected, line was rotated through 90°, using the point of bisection as a center of rotation.

The results are represented in Tables V, VI and VII and are represented graphically in the corresponding curves, Figs. 3, 4 and 5.

Since the four positions of the figure naturally divide themselves into two pairs, the vertical and the horizontal, the results for the four sets are grouped into two sets, namely, 1–3 and 2–4. For our present purpose such combination offers no objection. The results are expressed in terms of percentages, based upon the standard, 114 mm. Each record shows the average per cent of illusion for the day, with the per cent of mean variation.

The records are the gross results, without the elimination of the illusion of the vertical or any analysis. They simply mean that, under a given condition, the variable line was selected so much too short, or too long as the case may be. The minus sign denotes that the variable was selected longer than the standard.

D. H. was a freshman student in the university. She was a careful and painstaking observer of more than average intelligence. She had been a pupil of the experimenter in the preparatory school. This made her feel at home with the experimenter and favored a natural and docile attitude without any feeling of restraint. She did not know that an illusion was being measured nor did she have any specific knowledge about illusions. She regarded the experiment as an opportunity for accurate sense training and expected to acquire skill.

As the aim was to determine what mere persistent effort, without information, would accomplish, no introspections could

TABLE V. (D. H.)

Day	⊥ and T %Il.	⊥ and T %m.v.	⊣ and ⊢ %Il.	⊣ and ⊢ %m.v.
1	22.8	3.1	11.1	3.2
2	25.3	2.1	8.9	2.1
3	22.7	1.4	7.5	2.4
4	21.5	2.7	6.4	2.0
5	19.7	0.4	7.8	1.6
6	22.7	1.6	9.9	1.7
7	22.3	1.5	10.4	1.5
8	21.9	2.4	8.9	2.2
9	22.5	2.4	3.6	2.0
10	18.8	1.3	7.7	1.7
11	20.3	1.9	7.9	1.4
12	21.3	1.9	4.7	2.5
13	22.3	1.2	7.1	1.7
14	22.6	2.3	6.8	2.2
15	22.9	1.6	6.8	2.1
16	23.6	1.1	4.4	1.6
17	23.0	1.2	4.7	1.0
18	23.2	1.3	5.7	1.3
19	24.5	1.6	5.8	0.9
20	26.3	0.0	4.9	1.2
Ave.	22.5	1.6	7.0	1.8

TABLE VI. (T. S.)

Day	⊥ and T %Il.	⊥ and T %m.v.	⊣ and ⊢ %Il.	⊣ and ⊢ %m.v
1	22.8	3.1	0.1	3.8
2	21.2	1.8	− 2.6	1.8
3	18.8	1.6	− 4.8	1.8
4	19.2	1.8	− 5.7	2.2
5	14.6	1.3	− 7.1	1.4
6	15.4	1.3	− 6.6	1.6
7	14.3	1.7	− 4.8	1.6
8	13.7	1.8	− 6.0	1.8
9	13.2	1.5	− 5.1	1.5
10	11.5	1.1	− 7.7	1.5
11	9.5	0.7	− 9.3	1.5
12	9.9	1.5	− 7.1	1.1
13	9.9	1.3	−10.1	2.2
14	9.9	0.7	−12.2	1.1
15	10.2	1.3	−11.5	1.8
16	10.2	1.3	− 9.3	1.5
17	11.5	0.8	−11.3	1.6
18	11.7	1.3	−15.0	1.3
19	13.3	1.3	−14.1	1.7
20	16.8	1.9	−14.4	1.7
Ave.	13.9	1.5	− 8.2	1.7

TABLE VII. (C. E. S.)

Day	⊥ and T %Il.	⊥ and T %m.v.	⊣ and ⊢ %Il.	⊣ and ⊢ %m.v.
1	8.5	1.8	− 6.0	2.7
2	6.6	2.2	− 9.6	2.1
3	5.8	1.6	− 7.6	1.9
4	1.1	1.6	−11.5	1.2
5	1.1	1.7	−10.8	1.7
6	− 0.4	2.3	− 9.3	1.3
7	1.7	2.0	− 7.3	1.7
8	6.2	1.9	− 6.0	1.3
9	5.3	1.1	− 6.4	1.6
10	7.1	1.2	− 4.2	1.2
11	8.0	1.0	− 4.6	1.8
12	7.1	1.2	− 7.9	1.1
13	4.7	0.6	−11.0	1.4
14	4.4	0.7	−10.8	1.9
15	3.8	2.0	−11.3	1.2
16	1.3	1.1	−14.1	1.0
17	1.1	1.5	−12.1	1.3
18	0.7	1.0	−14.8	1.1
19	0.9	0.9	−15.0	0.9
20	1.3	1.3	−13.9	1.3
Ave.	3.8	1.4	− 9.7	1.5

be asked for until the series had been completed and but little was volunteered.

Reference to Table V and Fig. 3 shows that for the vertical positions of the variable her average gross illusion on the first

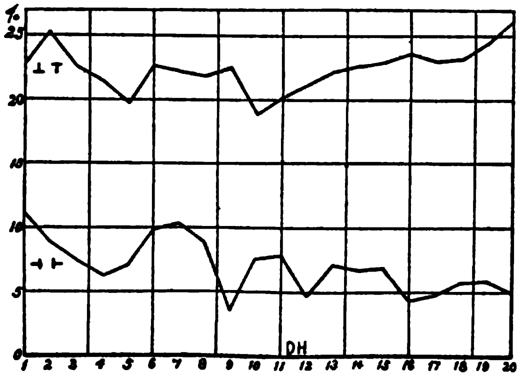


FIG. 3

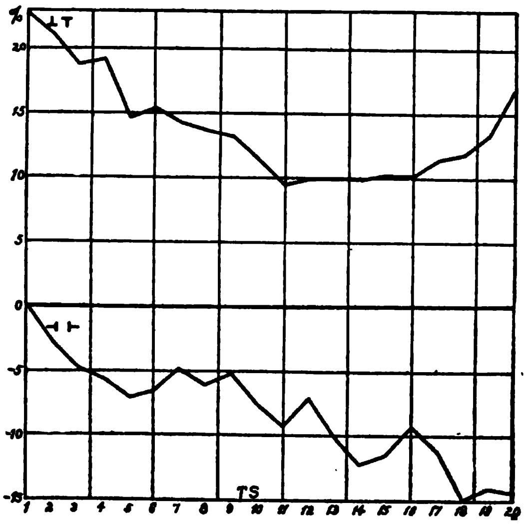


FIG. 4

day was 22.8% and, on the last day, 26.3%, there being a slight decline for the first ten days and a somewhat larger rise during

the second ten days. The average illusion is 22.5%, the minimum being 18.8% (tenth day) and the maximum 26.3% (last day). The table shows that the mean variation is small and regular.

For the horizontal position, the general average for the twenty days is 7%, the maximum, 11.0%, being on the first day and the minimum, 3.6%, on the ninth day. On the whole there is a slight general tendency in the direction of decline of the illusion. This decline but little more than offsets the increase shown for the vertical position.

To obtain a measure of her normal capacity in visual perception of space when no illusion was involved, she was tested

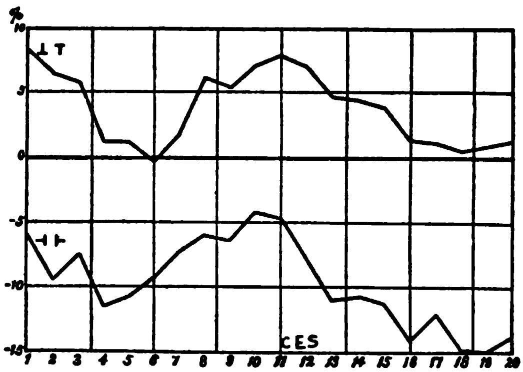


Fig. 5

on making one plain line equal to another, the two lines being in the same direction and end to end. With a line 114 mm. long, thirty trials resulted in an average error of 0.6% with a mean variation of 1.4%, which is a good record.

At the conclusion of the series, her illusion of the vertical was also measured with plain lines. Here the method of production was employed. The illusion of the vertical, when the variable line was in the horizontal position, averaged 16%; but, when the variable line was in the vertical position, she showed a strong tendency to correct for the illusion of the vertical. This tendency has been demonstrated before and is

due to the fact that the effort to adjust the vertical line makes the conditions for the illusion focal in consciousness, which is not the case in the other position. We may therefore take the record for the horizontal position as the truer record of her illusion of the vertical. This procedure is the more justified in view of the fact that the measurement was made after the training had been completed.

The attempt to eliminate the illusion of the vertical in cases like this is fraught with many dangers of error; there are a number of known, and doubtless also unknown, factors which should enter into our calculations, but we may make a gross elimination and bear in mind that the result is subject to minor corrections. Making the illusion, in round numbers, 15%, we find the data for the two positions self-consistent; for 22% minus 15% leaves a residual of 7% as the measure of the T-illusion in the vertical position; and, 15% minus 7% leaves a residual of 8%, which is the average record for the horizontal position.

We must guard against being misled by the difference in the level of curves in the charts. There is danger of assuming from a glance at the chart that the T-illusion is greater in the vertical position than in the horizontal.

For this observer, then, the T-illusion amounts to about 7%; in one position of the figure, there is a small increase and in the other, an equal decrease in the gross illusion. Therefore we may say roughly that the illusion is comparatively small and is uninfluenced by training without knowledge.

Observer D. H., according to remarks during the experiment and careful examination afterward, maintained a naïve attitude throughout the whole training. She followed her natural intuitive tendencies and reported her impression of the figure at each exposure without knowledge of danger or serious suspicion of sources of error. She trusted her eyes. Indeed she used different methods and commented on the reasons for change but spoke of them merely as "difficulties," giving the impression that they spurred her on to use her eyes carefully. She thought that she was learning how to see the figure most effectively, her confidence in her judgments increased and she felt that she was improving with the practice.

The data in hand would seem to justify the following interpretation of her case: She had a large illusion of the vertical which probably was not allowed for, or affected by the practice; she had a small T-illusion, probably due to the small and constant motives, which also remained practically constant throughout the practice. The practice resulted in a slight decrease in her mean variations for the daily records.

T. S. was a freshman, not exceptionally bright, but a faithful student who always secured good records in his class work. He knew nothing of psychology and approached the subject in the same naïve attitude as the foregoing observer, except that he was fully aware of the illusion of the vertical. He was asked not to make any allowance for this but simply to trust his eyes regardless of any theories he might have.

He started with a gross illusion of 22.8% which rapidly decreased to 9.5%, on the eleventh day, and then gradually increased to 16.8%, on the last day, making an average of 13.9%. In the horizontal position, his record starts with 0.1% and falls gradually to −14.4% on the last day, the average being −8.2%.

Tests for accuracy in space perception when no illusion is involved, as, e. g., in the above test on Observer D. H., show that in this respect he has greater ability than the average male university student. He made an average error of .2%.

Tests for the illusion of the vertical with plain lines, at the end of the series, revealed a strong illusion of the vertical—about 12%. As this record was obtained after the training series, it is safe to assume that the illusion of the vertical had not decreased much in the training. If we assume the conservative estimate of 10% as a constant illusion of the vertical, the T-illusion began with a force of 12.8% and fell off rapidly and steadily, during the first ten days, until it was eradicated; it remained absent for about six days and then gradually returned during the last four days.

Making the same allowance of 10% for the illusion of the vertical in the other position, we find that there is a very consistent parallel in the force of the T-illusion for the first sixteen days. During the last four days there is an end to this correspondence.

The general conclusion, then, would be that, this observer starts with a strong T-illusion which gradually passes away but returns with about half its original force in the vertical figure and is overcorrected at the same time in the horizontal figure.

Observer T. S. revealed by his remarks during the experiment and by examination after the experiment that he took an attitude of speculation and developed several theories which undoubtedly influenced him, although he tried to follow the instructions to make no allowance for any theory of sources of error. Thus, he learned by comparing different methods of judging the lines, that the bisection had the effect of shortening the line and he estimated this error to be possibly 20%. As a matter of fact, that motive does not amount to more than three or four per cent. This then, was a discovery of one of the true motives for the illusion, but it was overestimated and this overestimation led to the speedy decline in the record. Then, he developed a theory that the horizontal variable looks shorter than it really is, which is contrary to the T-illusion motive. He admitted a temptation to select a longer line on this theory, which would again tend to reduction of the illusion. This theory was, of course, based upon his knowledge of the illusion of the vertical and should be properly interpreted as resulting in a correction for the illusion of the vertical rather than an overcorrection for the T-illusion. This observer was aware that he got different results by different methods but the large changes in the curve are not due to any one method, because he changed methods frequently.

C. E. S. knew the details of the theory and the conditions of the experiment. He made an effort not to make any allowance for any of the known motives of illusion. From a former training series in the illusion of the vertical, he expected that to be about 6%. He expected the T-illusion to be considerably larger than that.

The gross illusion in the vertical position starts at 8.5% and falls rapidly, reaching zero on the sixth day; then it rises at about the same rate and reaches its original force on the eleventh day, and then falls again, reaching close to zero on the last five days.

The direction of the curve for the horizontal position is

fairly parallel to the curve for the vertical position. But the signs are minus, i. e., the horizontal line was made too long and the numbers in the low portions of the curve are too large to be expressions of the illusion of the vertical. This gives a clue to the interpretation of the large waves in both curves; namely, placing the normal illusion of the vertical constant at 6%, the drop below that number in either curve, regardless of sign, represents overcorrection for the T-illusion. This observer not only started with the T-illusion practically eliminated, but over-reacted against it unconsciously.

He represents the type of observer for whom the T-illusion is practically absent on account of training in accuracy of observation. But he started the training not knowing this, supposing that his records would show a decided T-illusion. It is well known that a conviction of that sort is almost sure to show itself subconsciously in some way. Here it resulted in periodic tendencies to overcorrect.

Although full notes are at hand, it is not easy to account in detail for these fluctuations. Of course, the observer did not know that there were any fluctuations until after the series was completed. From comparison of methods, chiefly the three mentioned, he concluded that the first (unanalyzed impression) would lead to the largest illusion, and the third (turning one half of the bisected line upon half of the other) was the most exact. In difficulties he used all three methods although he gave most weight to the third. But there is no traceable connection between the development of method and the notes on changes in method to correspond to the large waves in the curve.

After the nineteenth trial, he states:

"I cannot get myself to accept the third method 'straight' because it differs so much from the second. Still, at the present time, I approach the third method requirement more nearly than the second. I really believe that if I followed the third method rigidly there would be no error. I feel that there must be an illusion due to the bisection of the variable line which counteracts the T-illusion."

In the final introspection he says:

"I have continually struggled to remain in a naïve state of perception and to avoid making allowances. Thus, e. g., I have not figured out what the balance of all the illusions ought to be when the vertical line is in the horizontal position.

Still I have continually been conscious of a sort of allowance for the illusion of the vertical and the T-illusion, not in the sense of correcting fully for them, but in the sense of bearing in mind that there is a deceptive appearance. The judgment always impressed me as being very complicated and it has been difficult to keep the various factors constant. About the middle of the series I was particularly pleased with the 'Horizontal-left' figure. I felt that my judgment must be about right and thought that the two illusions probably cancelled. But the last few days I have grown more helpless and feel the greatest difficulty in this position."

An interesting index to this observer's orientation is obtained from a series of notes made from time to time saying which figures seemed to him to look exactly right and estimating what he thought might be right after corrections. On the fourth day he thought that the line which was −13% was probably the 'equal' figure in the vertical position. His record for that day was − 11.5%. On the fifth day he selected the −13% figure again as the 'equal,' *to his eye*, but estimated that, owing to the presence of the illusions, it was probably actually −4%. On the fourteenth day he was sure that the −9% figure was the true 'equal' figure, after due allowances for the illusions. We see in all three of these cases that the observer was under the impression that he had not corrected for the motives of the T-illusion, the error of his estimates being 13% in the first case, 9% in the second case, and 9% in the third.

To bring together the conclusions obtained from this inspection of the three records, we may say: One observer had a medium T-illusion which remained unaffected by practice; another observer started with a strong T-illusion which rapidly passed away, but returned in part for one position and resulted in a decided overcorrection in the other; the third observer had no T-illusion at the beginning but periodically over-reacted against the motive.

A general review of these facts would lead us to conclude (1) that the T-illusion may be due to either or both of two types of motives: first, failure to take a discriminative attitude toward the figure and, second, the presence of such more rigid motives as those which condition contrast and the underestimation of a bisected line. Observer D. H. was probably influenced only by the second type of motives, Observer T. S. by both, and

Observer C. E. S. only by the second. But C. E. S. labored under an exaggerated estimate of the force of these motives of the second type. (2) Where the motives of the second type are not known, the T-illusion is not likely to be affected by practice. (3) Where the motives of the first type are present at the beginning, they rapidly disappear with practice. (4) Suspicion of a large motive for illusion leads to unconscious correction.

It is fairly certain then, also, that wherever the T-illusion appears in very great force on first trial, it will decline with practice whether the observer proceeds with or without knowledge in the practice. Or, perhaps more to the point, the molives of the first type disappear the moment the observer takes that serious discriminative attitude which he naturally assumes in entering upon a training series.

The difference between the results for the members of each pair of figures may be noted in passing. In the horizontal position the 'left' line is made approximately one per cent shorter than the 'right.' This is in accord with the known tendency for equal distances to appear greater when at the left than at the right. In the vertical position the difference is greater. The 'up' line is made fully three per cent shorter than the 'down.' This is also in accord with the known tendency to overestimate the lower portions of a figure. In both positions the point of union of the two lines is naturally taken as the center of gravity of the figure.

III THE MUELLER-LYER ILLUSION.

MEASUREMENTS BY EVA CRANE FARNUM.

The conventional double fledged Mueller-Lyer figure was used in this study of practice-effect. Two series of training were run parallel, one by the method of production and the other by the method of selection.

In the method of production, Judd's Mueller-Lyer apparatus as described in The Yale Studies, N. S., I, 68–9, was used. This apparatus enables the observer to vary one of the base

lines by both coarse and fine settings without otherwise disturbing the figure. The records are made automatically.

In the method of selection, the complete figure was drawn on a series of large cards, the base line in one section being varied by three-millimeter-steps in the series of cards. The mode of procedure was identical with that described for the T-illusion above. A fresh start was made after every two complete determinations, the object being to prevent the observer from inferring what relation the present judgment would hold to a foregoing one.

The constant base line was 100 mm. long, the angle lines 30 mm., and the angles of these with the base line 45° or its complement. The section with converging angle lines was kept constant and always at the left by both methods.

The two methods were used together in order to make the practice as free as possible from the contingencies of method. The production method is rapid but is always subject to dangers of accessory parts in the figure and disturbing tendencies in movement. The only real dangers in the method of selection lie in the possibility of suggestion by the order of the presentation of cards and the possibility of identifying cards. The latter danger was guarded against by continually changing cards, and the former was eliminated by the order of procedure mentioned above. It is absolutely essential, in a test of this sort, that the observer shall have no means of determining objectively or by inference what the actual proportions are.

A day's practice consisted in twenty settings by the method of production and twenty complete determinations by the special method of selection. As each complete determination by this latter method involved from four to seven separate judgments, it afforded the largest practice, about one hundred and thirty judgments a day. After the first day or two, all this could be accomplished in thirty or forty minutes, if there was no interruption or rest.

Four observers engaged in the training; two for twenty-four, one for twelve, and one for thirty-five days. These experiments were made in 1906.

The results are condensed into Tables VIII, IX, X, and XI,

which give the record for each method, with mean variation, for the successive days. The method of production is designated by *P* and the method of selection by *S*. The figures are averages for the day and are expressed in percentages.

TABLE VIII. (C. E. S.)

	S		*P*	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Feb.				
23	9.1	1.1	12.1	1.1
24	10.0	1.0	10.5	1.4
26	10.7	1.5	8.7	1.5
27	10.0	0.6	8.9	1.3
28	8.7	0.9	7.1	2.0
Mar.				
1	7.3	1.1	5.6	1.2
2	5.7	0.3	5.1	1.0
3	4.5	0.9	3.2	1.2
5	3.2	1.1	2.9	1.8
6	3.1	0.7	2.6	1.4
7	3.5	0.7	4.1	1.5
8	3.4	0.8	3.4	1.2
9	2.0	0.9	3.2	1.1
10	1.4	1.0	0.5	1.2
12	1.4	1.2	− 0.7	1.2
13	1.2	0.7	− 1.4	1.4
14	0.4	0.5	− 1.1	1.0
15	1.2	1.1	− 1.1	1.1
16	− 1.4	1.0	− 2.9	1.6
17	0.6	0.9	− 3.8	1.2
19	0.1	0.4	− 2.1	1.1
20	0.1	0.5	− 4.5	1.2
21	0.5	0.8	− 3.6	1.4
22	0.4	1.2	− 3.5	1.7

TABLE IX. (D. S.)

	S		*P*	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Feb.				
26	11.9	1.3	12.9	3.3
27	10.7	0.9	14.0	1.9
28	12.6	1.8	14.3	2.0
Mar.				
1	10.3	1.3	9.8	1.4
2	7.2	1.4	8.6	1.8
3	6.6	1.1	7.0	2.0
5	2.2	1.4	3.7	1.1
6	4.7	1.9	6.5	1.4
7	4.5	0.6	5.2	1.3
8	4.2	0.8	3.8	1.0
9	1.2	1.0	4.4	1.1
10	1.0	1.7	5.2	1.7
12	0.0	0.5	4.1	1.2
13	− 0.1	0.4	7.3	1.0
14	0.4	0.5	6.7	1.0
15	− 1.2	1.7	3.8	2.3
16	− 2.8	0.4	0.8	0.7
17	1.1	0.8	2.6	0.8
19	0.3	0.6	0.1	1.0
20	0.2	0.3	1.1	1.2
21	− 1.1	1.2	1.9	1.2
22	− 3.3	0.9	1.0	0.9
23	− 2.3	0.9	2.7	1.0
26	− 1.1	1.0	1.7	1.5
27	− 0.7	0.9	3.1	1.1
28	− 1.9	1.1	3.0	0.9
29	− 1.9	0.7	2.2	1.2
30	− 2.3	1.1	2.1	0.6
31	− 4.3	0.9	1.4	0.8
Apr.				
2	− 3.3	0.8	1.8	1.1
3	− 4.1	0.7	1.2	0.5
4	− 3.5	1.0	3.0	1.3
5	− 2.7	1.2	2.0	0.5
6	− 2.9	1.1	2.2	0.5
7	− 2.9	1.0	1.9	0.6

TABLE X. (T. P.)

	S		P	
Date	%Il.	%m.v.	%Il.	%m.v.
Mar.				
9	15.7	2.1	21.5	2.8
10	18.6	1.2	16.8	0.9
12	20.4	1.1	16.5	0.8
13	19.5	1.0	19.2	0.5
15	21.3	1.4	19.7	0.5
16	24.7	1.2	20.4	0.8
17	21.9	1.2	20.1	0.4
19	21.9	1.0	21.9	0.7
20	22.0	1.1	21.0	0.6
21	22.0	1.2	21.7	0.8
22	22.8	1.0	21.3	0.7
23	22.0	0.9	21.8	0.7
24	21.7	0.9	21.7	0.7
27	21.5	1.2	20.9	0.4
28	21.0	0.4	20.8	0.4
29	20.1	1.1	20.5	0.5
30	19.6	1.1	20.9	0.9
31	19.5	0.9	18.4	0.8
Apr.				
2	19.9	1.0	19.8	0.6
3	19.6	1.0	19.7	0.4
4	19.3	1.0	19.9	0.6
5	19.2	1.1	19.9	0.6
6	19.3	1.0	19.6	0.8
7	19.0	1.0	19.7	0.7

TABLE XI. (J. A. M.)

	S		P	
Date	%Il.	%m.v.	%Il.	%m.v.
Mar.				
28	25.5	1.3	26.7	1.5
29	22.4	1.2	26.6	2.8
30	14.5	2.3	26.0	2.2
Apr.				
2	11.7	1.4	24.2	1.7
3	10.4	1.4	19.0	1.5
5	6.2	1.9	19.0	1.7
7	5.7	1.7	16.8	1.7
9	3.2	0.2	17.2	1.6
10	2.1	0.2	15.1	3.0
11	2.9	0.2	16.1	1.8
12	2.9	0.1	11.9	2.9
13	3.4	0.4	11.9	2.1

A minus sign indicates that the illusion was reversed. The same results are represented graphically in the curves. Figs. 6, 7, 8, and 9.

C. E. S. who was familiar with all the details of the experiment, started with a fairly strong normal illusion and this was gradually eradicated by the practice. The illusion disappeared on the fifteenth day by the method of production, and on the nineteenth day by the method of selection. The records for the two methods run fairly parallel until the last one-third of the series, when a progressive deviation begins. The records for the method of selection remain on a level, near zero for the last third of the series of days; but the other record continues to fall and is increasingly negative from the fifteenth day on.

The result was astonishing to the observer when he saw it after the series had been completed. From many years of experience with this illusion, and from practice on other illusions, he had been led to the conviction that no improvement would take place without conscious change in the perceptual

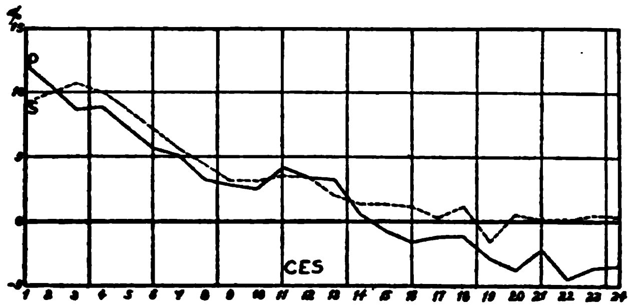


FIG. 6

attitude; and here he had remained in the same perceptual attitude toward the motives of the illusion, so far as it could be analyzed by introspection, from the second day to the end. He was under the impression that his normal illusion would amount to about 6% and that it would remain constant throughout.

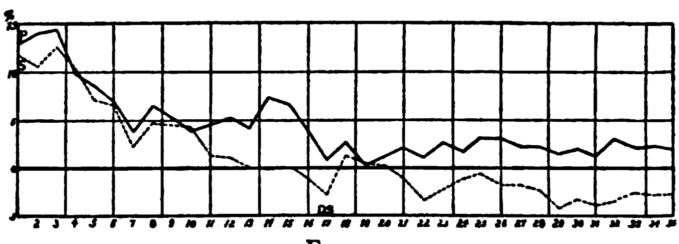


FIG. 7

From one point of view, this conviction was favorable to the reduction of the illusion for it kept the observer in that complacent attitude in which the perceptual process might adapt itself to the confronting difficulty without rousing consciousness of adaptation.

His method of judging was to fixate the middle joint and judge the two sections with the eyes at rest on this point, but

the act usually resulted in sweeping eye movements in both directions after the first fixation.

The difference between the *P* and the *S*-records in the latter part of the series was predicted by the observer during

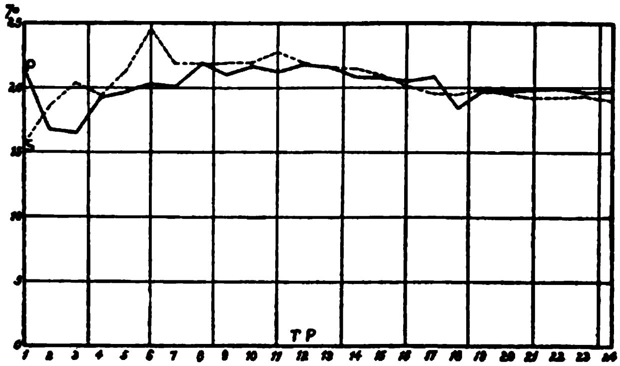


Fig. 8

those experiments. He recorded that the line of overlapping in the cards in the method of production was a source of confusion and he estimated that this might lead to the production

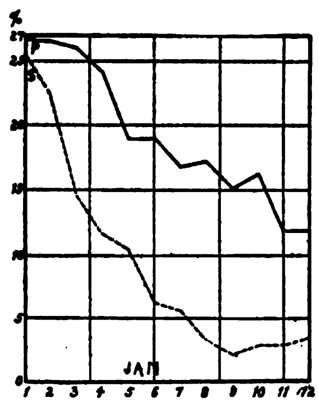


Fig. 9

of a longer line because a single interruption of a line leads to underestimation of that line. It is however difficult to see why the same principle should not have operated in the earlier part of the series when he was not aware of it as disturbing.

D. S. was also a trained observer thoroughly familiar with all the conditions of the experiment. So far as the effect of training is concerned, his records resemble those of the foregoing observer. The parallel is particularly close in the *S*-records. There is a large divergence in the results for the two methods, and it is in the opposite direction to that found for the former observer. Taking a mean between the two records, we may say that the illusion falls off gradually during the first half of the series and remains practically eliminated during the second half.

The main drop in this curve is explained by the introspective notes on method. For the first three days he recorded that he did not try to follow any particular method or contrivance but naturally took a glance at the figure as a whole and then sought a general impression as to the balance of the two parts to be compared. But on the fourth day he noticed that, "if I try to disregard the end-lines and center my attention upon the horizontal line, the left part has to be considerably longer than by the previous method in order to appear equal to the right." From the fourth day to the seventh, inclusive, he oscillated between these two methods but considered the latter the more satisfactory. On the eighth day, he fully adopted the second method and continued it to the end. During the transition period, he stated that the change of method must have resulted in a decrease in the illusion.

This record again illustrates the danger of constant errors, peculiar to the method of experimenting, entering into the record. Nothing in the introspections, in the critical review of conditions by the observer at the close of the experiment, or in the known conditions of the technique explains the divergence between the two records. We have no means of knowing which if either is the true one, further than the fact that the *P*-method is more complex and therefore more liable to error than the other.

The observer did not think that he had been influenced in his judgments by knowledge of the illusion. The force of the illusion is, then, accounted for in part by his method, or lack of method, for the first three days. The rapid decline following

is accounted for by the change of method. The further lowering is probably due to developed capacity for disregarding the accessory lines. The fact that the observer felt sure that the illusion had not been eliminated by the practice may have something to do with the overcorrection shown by the *S*-method.

T. P. was entirely naïve and uninformed so far as the psychology of illusions is concerned, and remained so throughout the experiment. He supposed that the accessory lines were there merely for the purpose of making the task of training more difficult, and never suspected the presence of any illusion. He was fully aware that he was to compare the base-lines only, but he was not aware that the accessory lines would influence his judgments. He therefore proceeded in the same way as a trained observer would in comparing two plain lines. It is easy to understand that it is no small achievement on the part of the experimenter to keep an observer so free from suggestions for twenty-four periods of experiment. This observer was a graduate student, with major in political science, extraordinarily faithful in his task, and confident of great good to result from his efforts.

He started with the strong illusion characteristic of those who are not aware of its existence and, although there are deflections in the curve, we may say in general that it remains at the level of about 20% without any tendency to change as the result of the practice. The mean variation is exceedingly small in comparison with the magnitude of the illusion, and the records by the two methods agree remarkably well.

A. M. was a graduate student in modern languages. His attitude may be expressed by quoting from notes made by him after the experiment had been completed. He says:

"I knew that 'things were not what they seemed,' before I commenced the experiments. But I had no idea as to what caused the illusion. I tried to estimate the length of the lines just as they appeared to me without making any allowance for any 'fake' I might imagine to exist. After a while the figures appeared different (?) without my making any allowance at all for the illusion. I did not attempt to figure out any system and did not think of the figures between experiments. The fact that I knew of the illusion may have unconsciously influenced my judgment, but I tried to guard against that."

The experiment was unfortunately interrupted, but it was carried far enough to reveal his type of reaction clearly. He started with the strong illusion of the uninformed but rapidly decreased this. The interpretation is undoubtedly to be found in the fact of a progressive adaptation of his mode of grasping the figure in a way favorable to isolation of the lines compared, but he made no effort to analyze the process. The difference between the results by the two methods is large.

Here again the different observers reveal different types of practice effect. In general the results may be summed up as follows: When the observer proceeds to the experiment with full knowledge of the conditions and does not expect the illusion to disappear, it does pass away, and without leaving any introspective evidence of the change; when the observer proceeds to the experiment without any knowledge of the illusion, and is not led to suspect any illusion, the force of the illusion remains unchanged throughout long continued practice; and, when the observer suspects the illusion but has no definite knowledge of its cause, it tends to disappear as in the cases of specific knowledge of it.

The difference between the results for the two methods of measurement employed shows the danger of ascribing to practice in general what may be due to peculiarity in method, and how practice may lead to improvement by one method of estimation and not by another.

Two years after the above training series had been completed C. E. S. and D. S. repeated the test to determine the effect of the long interval upon the practice gain. C. E. S., taking twelve complete determinations by the method of selection, revealed, an illusion of 12% with a mean variation of 1.4%. D. S., taking one hundred trials by the method of production, gave an average of 9%. The illusion had therefore returned to the approximately normal force that it had before the training. Unfortunately we have not yet had opportunity to repeat the training to determine to what extent the second training would profit by the first.

IV. THE ILLUSION OF DISTANCE BETWEEN CIRCLES.

Measurements by Raymond W. Sies.

The linear distance between two circles a moderate distance apart is overestimated. This is undoubtedly a form of the Mueller-Lyer illusion but it probably involves other motives than those ordinarily operating in the conventional form, as in the above experiments.

In the following experiments upon the effect of practice on this illusion the method of selection was employed as described in the section above, on the T-illusion. The standard figure consisted of two circles, each 114 mm. in diameter, the space between them being equal to the diameter of a circle. This figure was drawn on a series of cards, the inter-space being varied in successive cards by three-millimeter steps from 75 to 135 mm. The task was to select the card in which the distance between the two circles seemed to be equal to the diameter of a circle. Every effort was made to eliminate suggestion or information in regard to the illusion from the apparatus and method.

Three observers, representing as many different types of preparation, engaged in the test. Each observer made sixteen complete determinations each day for twenty successive days, Sundays excepted. This amounted to about one hundred judgments a day for each observer. The tests were equally distributed for the horizontal and the vertical positions.

Table XII shows the results for the three observers, giving the average illusion with its mean variation of the successive days for each of the three observers. Reversal of the illusion, i. e., underestimation of the distance between the circles is indicated by the minus sign. Fig. 10 represents these records graphically for the vertical position and Fig. 11 for the horizontal position.

C. E. S. is the trained observer who has taken part in the preceding experiments. His training on the illusion of the vertical, taken five years before, had resulted in no evidence of progressive gain from the practice. His training on the illusion of cylinder length had resulted in the same way. His train-

ing on the T-illusion had shown no appreciable progressive gain because the illusion was practically absent at the beginning of the training. The foregoing series of training on the Mueller-Lyer illusion was in progress and had reached the fourteenth day when the present series began, the observer of course being ignorant of what record he was making in the former until that

TABLE XII.

Day	Observer C. E. S.				Observer F. V.				Observer R. M.			
	Vertical		Horizontal		Vertical		Horizontal		Vertical		Horizontal	
	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.
1	7.2	5.0	0.8	2.8	2.6	3.7	1.5	1.0	17.8	2.7	5.9	3.1
2	4.1	1.9	−1.7	0.4	2.6	3.7	1.1	0.6	18.9	1.5	7.1	4.3
3	3.8	1.6	0.2	2.2	−0.2	0.3	−1.3	1.8	14.5	6.0	6.6	3.8
4	3.2	1.0	0.4	2.4	−1.1	0.7	−2.0	2.5	16.1	4.3	2.8	.0
5	2.6	0.4	−0.8	1.2	−3.9	3.5	−3.9	4.5	13.5	6.9	3.6	0.8
6	3.2	1.0	−1.5	0.5	−1.1	0.7	−0.4	0.9	14.6	5.8	−0.7	3.5
7	1.7	0.5	−2.8	0.8	−2.1	1.7	−0.4	0.9	14.6	5.8	−1.8	4.6
8	2.6	0.4	−2.3	0.3	0.0	0.4	0.5	0.0	16.6	3.9	−1.7	4.5
9	3.3	1.1	−1.8	0.2	0.4	0.8	0.5	0.0	17.1	3.3	0.0	2.8
10	1.8	0.4	−1.5	0.5	−2.6	2.2	−0.8	1.3	21.2	0.8	0.8	2.0
11	1.8	0.4	−3.0	1.0	−0.5	0.1	1.5	1.0	23.9	3.4	2.3	0.5
12	1.8	0.4	−3.3	1.3	1.0	1.4	0.2	0.4	22.2	1.8	2.3	0.5
13	1.7	0.5	−3.9	1.9	−1.7	1.2	−0.8	1.3	21.1	0.6	1.1	1.7
14	3.0	0.8	−1.1	0.9	−1.0	0.5	−0.2	0.7	21.7	1.3	2.0	0.8
15	2.0	0.2	−2.8	0.8	−1.0	0.5	1.3	0.8	24.8	4.4	2.3	0.5
16	0.2	2.0	−3.0	1.0	0.5	1.0	3.0	2.5	25.7	5.2	3.9	1.1
17	1.1	1.1	−2.5	0.4	0.8	1.2	3.6	3.1	24.8	4.4	4.7	1.9
18	0.4	1.8	−3.8	1.8	0.2	0.6	1.8	1.3	25.4	4.9	4.1	1.3
19	−1.8	4.0	−2.6	0.6	−0.5	0.1	3.3	2.8	27.0	6.5	4.3	1.5
20	−0.5	2.7	−2.6	0.6	−0.5	0.1	2.3	1.8	27.6	7.2	5.9	3.1
Ave.	2.2	1.4	−2.0	1.1	−0.4	1.1	0.5	1.4	20.4	4.0	2.8	2.1

series was completed, which was on the twelfth day of the present series.

The outcome of the training in the first three types of illusion had led us to assume that the overlapping of these two series would not interfere. The records show that this assumption was wrong and unfortunate. C. E. S. supposed that the illu-

sion at the beginning of this series would be about 8% for the vertical position and about 4% for the horizontal position. This estimate was based upon knowledge of the average illusion for students in the laboratory as well as upon measurements

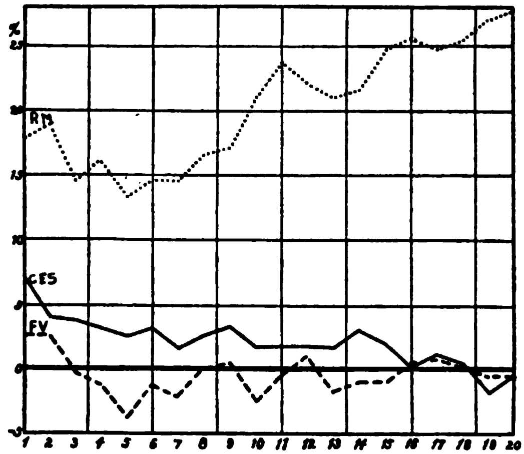


FIG. 10. Vertical.

made upon himself about seven years before. The results were therefore very surprising to him. They were doubly surprising, because after learning on the twelfth day of this series that the foregoing form the Mueller-Lyer illusion had tapered off with practice he had from that time supposed that the same process was going on for the present form of the illusion.

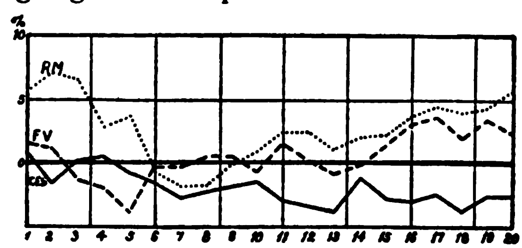


FIG. 11. Horizontal.

It is clear that there is a transference of practice-gain from the conventional form of the Mueller-Lyer illusion to this variant for the illusion is smaller at the beginning of this test than before the training in the former. The amount can not be stated as

the early measurements were made under different conditions. The observer's information of and surprise about, the outcome of the former series does not seem to have resulted in any objective evidence in the results after the day the information was obtained.

There is indeed a considerable illusion for the vertical position and this falls off rapidly during the first three days. The smallness of the error for the vertical position must not, however, be interpreted as evidence of efficiency only but also as the result of unconscious correction for the known motives of the illusion. This is demonstrated by the fact that there is a decided over-correction in the horizontal position. Although the small error in the vertical position remains fairly constant throughout, the series really ends with this also negative, which is further proof of the tendency to correct.

The following introspection was written by C. E. S. after the second day of training:

"The judgment on this figure is very uncertain. I notice three distinct methods of seeing the figure. (1) Seeing the whole figure in one sweep with an attempt to divide it into three parts without superposition of parts. (2) Comparing the diameter with the central space, allowing the limiting arcs of the circle to suggest bands of space about two inches wide, instead of a mere line. (3) Trying to image mere linear distances and superposing these. The third I think is the most effective but it is also the most difficult. The illusion seems to be greatest for the first, then the second is next, and the third is freest from illusion. Wherever the comparison is close I tend to use the third method.

"The circles appear distinctly oblong vertically. The lower circle seems to be larger than the upper. I usually use the lower. The right circle seems to be larger than the left. I tend to use the right.

"I had no basis for estimating the strength of the illusion at the start, if present at all. I am continually conscious of the direction of the illusion and should think that this would lead to a small illusion."

The second observer, F. V., was a sophomore of average intelligence, who knew of the illusion and some of the motives for it; but he was not a trained observer and it was impossible for him to make sharp distinction between what it appeared to be and what he estimated it to be with knowledge of conditions. He started with the illusion practically eliminated and made a very regular and consistent record with practically no illusion present in either vertical or horizontal position.

This record has but little value from the point of view of training. From measurements on variants of the illusion it was demonstrated that this observer was subject to the normal illusion when he was taken unawares. We should not conclude that he was dishonest, nor that he was a poor observer in other respects; but, having satisfied himself about the amount of the illusion before beginning the test, and knowing the tendencies present, he was not able to distinguish the logical estimate from the visual presentation.

The third observer, R. M., was also an undergraduate of strong ability, but he knew nothing of this illusion and remained in a naïve state of mind with reference to it throughout the test. The average illusion for undergraduates under the same conditions in a single test is 15% for the vertical position and 4% for the horizontal. His illusion is therefore 5 per cent above the normal in the vertical position and 1% below in the horizontale To be more specific, with reference to the vertical position he starts with an illusion which is about normal for observers of his class, this remains fairly constant for the first nine days, but in the last ten days, it increases 10%. For the horizontal position, he starts with an illusion above the normal, which falls off gradually to an overcorrection during the first eight days and then gradually returns so that the series of training ends with the same degree of illusion with which it began. The observer was greatly astonished at the results, but he could give no introspective account of changes which would account for these variations in his record.

On the surface the records of C. E. S. and F. V. are similar. C. E. S. undoubtedly reduced his illusion by training on the regular Mueller-Lyer figure but he also made unconscious correction and even over-correction. The first two or three days may also be interpreted as showing decrease with the training in this form. On the other hand F. V. did not have the advantage of practice, was not influenced by any foregoing training but by the knowledge of conditions which resulted in a confessed inability to take the strictly perceptual attitude. R. M. represents a type we have found in every series before—the person who does not know of the illusion and who, as a result, shows

a strong illusion which does not tend to disappear or diminish with practice.

GENERAL CONCLUSIONS.

The essential feature of these experiments lies in the demonstration of a number of factors which determine what effect practice shall have upon these normal illusions. Chief among these are the degree and the kind of knowledge of the illusion, the capacity for maintaining the perceptual attitude, speculative tendencies, the estimate placed upon the known illusion at the beginning of the training, the duration of the training, knowledge of progress, the effect of a long interval after training, and different types of motives for illusion.

The list of the cases in Table XIII is arranged as a partial aid in a review of the results. The Roman numerals refer to the series: I, the illusion of cylinder length; II, the T-illusion; III, the Mueller-Lyer illusion; and IV, the illusion of distance between circles. The observers are designated by their initials; v. and h. denote the vertical and the horizontal positions respectively; s. and p. designate the method of selection and the method of production respectively. 'Strength,' has reference to the strength of the illusion at the beginning of the training; 'Knowledge' has reference to the observer's knowledge of the existence and character of the illusion involved.

The illusion persists with undiminished force so long as the observer has no knowledge of its existence. This is illustrated by Cases I, O. W.; II, D. H.; and III, T. P.—as it was illustrated first in the study on the size-weight illusion and the illusion of the vertical mentioned in the introduction. We have found no exception to this rule.

Among observers who have knowledge of the illusion those who are best capable of maintaining the perceptual attitude (i. e., reporting what they actually perceive as opposed to what they may judge relations to be) are least likely to decrease the illusion by practice. This assertion is based largely upon the introspections and the internal evidence in those cases in which the observers had knowledge. The surest way of aiding the observer in maintaining the perceptual attitude is to find and

keep him ignorant of the existence of the illusion and free from suspicion of it. That was an easy task fifteen years ago, but is exceedingly difficult now in the face of popular knowledge of the illusion.

Of course, if one knows the illusion he can readily learn to make proper correction for it in a judgment. Such correction

TABLE XIII

Case		*Strength*	*Knowledge*	*Practice Effect*
I, C. E. S.,	v.	Small	Full	Not any
	h.	"	"	" "
I, L. F. S.,	v.	Not any	Partial	Overcorrection
	h.	Medium	"	Increase-decrease
I, J. O. D.,	v.	Medium	Partial	Not any
	h.	"	"	" "
I, O. W.,	v.	Very large	Not any	Great increase
	h.	" "	" "	Increase
II, D. H.,	v.	Small	Not any	Slight increase
	h.	"	" "	Slight decrease
II, T. S.,	v.	Medium	Partial, suspicion	Decrease-increase
	h.	Small	" "	Overcorrection
II, C. E. S.,	v.	Very small	Full	"
	h.	Not any	"	"
III, C. E. S.,	s.	Medium	Full	Complete reduction
	p.	"	"	Overcorrection
III, D. S.,	s.	Medium	Full	"
	p.	"	"	Decrease
III, T. P.,	s.	Large	Not any	Not any
	p.	"	" "	" "
III, A. M.,	s.	Large	Partial, suspicion	Decrease
	p.	"	" "	"
IV, C. E. S.,	v.	Small	Full	"
	h.	Not any	"	Overcorrection
IV, F. V.,	v.	Not any	Partial, speculative	Not any
	h.	" "	" "	" "
IV, R. N.,	v.	Large	Not any	Increase
	h.	Small	" "	Decrease-increase

is at first focal in consciousness but soon becomes so automatic that the closest introspection may not trace the correction process involved in the form of an allowance for the illusion. To prove that we can make conscious correction for the illusion would be a waste of energy; to assume that such correction could not be made in a normal individual would be absurd. We

are therefore interested only in the effect of practice upon actual perception, or what seems to be perception; but the judgment process shades so imperceptibly into the perception process that the task of distinguishing them becomes exceedingly difficult.

Some illusions may be eradicated with practice without leaving any conscious trace of the correction. See, e. g., I, L. F. S. v; III, C. E. S.; III, D. S. These are in accord with Judd's results on the Mueller-Lyer illusion and Cameron and Steele's[1] results with the Poggendorf illusion. The figure looks different at the end from what it did at the beginning of the series. But, so far as the records go, such reduction has taken place only for persons who know of the illusion. From this it is not necessary to assume that the decrease is due to conscious correction; it may be due to the ability to avoid a certain kind of eye movements, attention to accessories, etc.

The illusion may disappear at the very beginning of a training series merely as a result of the elaborate and discriminative adjustment for systematic observation, and without any practice. For all who know of the illusion, there is a tendency to begin the training series with a smaller illusion than would ordinarily be shown in a single test. The cases of small, or not any, illusion in the above list illustrate this.

When the illusion decreases as the result of practice, without the appearance of any change in attitude of the observer, the gain takes the form of the conventional curve of learning; the illusion tapers off gradually during one or two thousand trials, with normal fluctuations.

The gain made by training in a series like this is not retained permanently; it may be wholly or partly lost in two years.

Overestimation of the illusion which is supposed to be involved at the beginning of a training series tends to lead to correction. This is one of the clearest evidences of the failure to maintain the perceptual attitude. This is illustrated in I, L. F S. v; II, C. E. S.; and II, T. S., h.

[1] Cameron and Steele; "The Poggendorff Illusion," Yale Psychological Studies, N. S., I, 83.

In partial knowledge of a given illusion, those motives which are known are affected as in full knowledge of the illusion whereas those which are not known remain unaffected by the practice. See Cases I, L. F. S., h; I, J. O. D.; III, A. M.

Suspicion of the illusion is likely to result in erratic records, depending in part upon the rightness or wrongness and in part upon vacillation in the suspicion. Where the suspicion is specific it operates in the same way as knowledge. The increase in an already large illusion may be accounted for by the presence of an erroneous suspicion; e. g., I, O. W., v; and IV, R. M. Case II, T. S., may illustrate a vacillating suspicion with reference to the vertical position and a true and firm suspicion with reference to the horizontal.

Where the same motives are involved, the gain made by training on one variant is transferred to another variant while the results of the training are fresh. See the effect of Series III upon Series IV in the case of C. E. S.

In all these experiments the observers were kept completely ignorant of their records until the training had been completed. With progressive knowledge of one's records the results would undoubtedly be quite different. It would probably be impossible to maintain the perceptual attitude under such circumstances. The gain would, of course, be more general and more rapid.

The fact that observers who are working extensively with illusions tend to have comparatively small illusions would seem to show that the discriminative attitude of the trained observer, regardless of practice in any particular illusion, weakens some motives for illusion and wholly obliterates others. In many illusions trained observers may choose to regard the object in such a way as to obtain a strong illusion, a small illusion, or no illusion at all, at will.

There are undoubtedly two general types of motives for illusion: those which are due to lack of discriminative apperception of the task in hand, as in the large illusion in the T-figure; and those which are due to deeply ingrained misleading tendencies in the interpretation of sensory data, as in the illusion of the vertical. Such complex illusions as the Mueller-Lyer, the size-

weight, the cylinder length, and the T-illusion probably involve both. The force of the rigid sensory motives is also lessened by a keen discriminative attitude of the observer.

The practical value of experiments of this sort is evident. In these training series we have followed the making and unmaking of habits under conditions partly controlled and have traced the dominance and the vanishing of ideas in developing perception. We feel that the possibility of working out a serviceable system of laws governing the persistence and the elimination of normal illusions which enter into our ordinary sensory experience is as promising as it is urgently needed.

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JOHNS HOPKINS UNIVERSITY

HOWARD C. WARREN
PRINCETON UNIVERSITY

AND

CHARLES H. JUDD
YALE UNIVERSITY
(Editor of the Psychological Monographs)

Studies from the Psychological Laboratory of the University of Chicago

COMBINATION TONES AND OTHER RELATED AUDITORY PHENOMENA.

BY

JOSEPH PETERSON
Instructor in Psychology in Brigham Young University.

THE REVIEW PUBLISHING COMPANY
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PREFACE.

The first part of this monograph is devoted primarily to a critical exposition of the important theories of combination tones and a statement of the facts upon which they rest. This undertaking inevitably leads to the mention of a considerable number of closely related phenomena whose significance for general theory is often crucial. In view of the conditions prevailing in the literature of the subject, it has been thought expedient that this presentation should in the main follow chronological lines. The full analytical table of contents, together with the division into sections, will readily enable readers who so desire to consult the text on special topics. The second part of the monograph reports certain experimental observations made by the author on summation tones.

In the pages which follow several physical terms are used which are commonly understood and need no definition here. A few may be new to readers not familiar with the literature of acoustics. A "pendular (or pendulum) vibration" is a simple, or sinus-form, vibration. In several quotations the word "note," and sometimes "sound," occurs where the meaning is precisely that of the word "tone." "Interruption tone" is used synonymously with "intermittent tone." By "transformation of the primaries" is meant the process of superposition of vibrations explained by Helmholtz in his mathematical determination of the origin of combination tones. A combination tone may, of course, be either a difference tone or a summation tone.

Where p and q are used to designate the two primary tones, p refers to the higher and q to the lower tone. Frequently h and t are used for the higher and lower tones respectively: these are most commonly used now, especially by German writers. N is sometimes used for the lower and n' for the higher tone. Some writers have used n for the interrupt*ed* and m for the interrupt*ion* tone; others have used these letters

in the reverse order. Since quotations are so frequently made from various writers no exclusive use of any of these letters can well be adopted here.

In designating the octave of any tone, usage also varies. Some writers use c^2, *e. g.*, where others use c'', and still others c_2. In this paper the now more common usage, . . . C_2, C_1, C, c, c^1, c^2, c^3 . . ., has been adopted, c being 128 d. vib. I have taken the liberty to change all other markings into this system in the quotations. In the *experimental* part, however, where the Koenig forks were used exclusively, I have simply employed the designations used by Koenig, *e. g.*, Ut_3, Re_3, etc., Ut_2 being the same as c (128 d. v.).

In quoting from the French and German where the translation is my own, I have used single quotation marks ('——').

The theories of Rutherford, Waller, Hurst, Emile ter Kuile, Ewald, and others, have not been considered in this paper. In the opinion of the writer, they have contributed nothing to the subject immediately under consideration, *i. e.*, certain *secondary* auditory phenomena, but are, as yet, concerned essentially with the *primary* phenomena of hearing.

It is a pleasure, in this connection, to express gratitude and to acknowledge obligation to my teachers, Professor J. R. Angell and Dr. J. B. Watson, from whom I have received constant advice and patient criticism. I am much indebted to Mr. W. V. D. Bingham for kind suggestions and assistance. I also wish to thank my fellow students of the psychological laboratory, who served as subjects for my experiments.

J. P.

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COMBINATION TONES AND OTHER RELATED AUDITORY PHENOMENA

PART I.

HISTORICAL AND CRITICAL.

SECTION 1. Early Observations and Theories, Combination Tones, Beats, Resonance Phenomena, Interruption Tones and so Forth.

It is well known that up to Helmholtz's day difference tones were supposed to originate from rapid beats. We shall see that this view had taken its rise naturally from the fact that the vibration-number of the noticed difference tone equals exactly the number of beats of the generating tones.

Combination tones were discovered in 1745 by Sorge,[1] who apparently heard, usually, only those which corresponded in pitch to what is now called the first difference tone. Krueger suggests[2] that Sorge heard this tone only in cases where it is reinforced by coincidence with one or more of the combination tones of higher order, as in the case of the fifth or the major third.

The pitch of the first combination tone was estimated an octave too high, by the Italian violinist, Tartini (1754).[3] This tone is sometimes called Tartini's tone. Its true pitch was determined theoretically by Chladni in 1802,[4] for intervals which may be represented by $n:n+1$. Wilhelm Weber

[1] A. Sorge, *Vorgemach der musikal. Composition*, 1745–47.

[2] 'Zur Theorie der Combinationstöne,' *Phil. Studien*, XVII., 1901, p. 207.

[3] Krueger, *op. cit.*, p. 189; also Helmholtz, *Sensations of Tone*, third ed. of Ellis' trans., 1895, p. 62 a.

[4] Chladni, *Akustik*, 1802, p. 207.

(1829)[5] also speculated on the pitch of this tone, and agreed with Chladni. He also remarked—what in many cases is true—that double clangs whose vibration ratio does not depart too much from the true relations (wahren Verhältnissen), *i. e.*, slightly mistuned consonances, should give the difference tone (weakened) of the nearest consonant interval.[6] Krueger thinks that the tone Weber had in mind is the intertone (Zwischenton) later discovered by Stumpf.[7]

Weber is led to the conclusion that when the vibration-ratio lies as near to 5:6 as it does to 4:5, it should be possible sometimes for one to hear a Tartini's tone a third lower than the second octave below the lower primary tone, and sometimes a tone two octaves below the lower primary. 'Investigations have failed thus far,' he said, 'to discover both of these.'[8] On the previous page he states somewhat definitely the view earlier announced by Thomas Young. 'When, *e. g.*, 4 vibrations of the one wave-series fall into the ear in the same period of time as 5 vibrations of the other, there will be experienced by the ear, in every such period, one increase (Anschwellen) and one decrease in intensity of each tone, or, when these recur too rapidly to be perceived separately, a deeper tone will be heard (empfunden), which is equal in pitch to the tone which would result if the drum were actuated by vibrations 4 times as slow (4 mal langsamer) as those of the first wave-series. Such a tone is two octaves lower than the one generated by the first wave-series' (p. 218).

In the meantime Baron Blein, whose work[9] was brought to Weber's notice by Alexander Von Humboldt, had been working with vibrating cords, using the fundamental tone c^1 (256) and the other primary tone varying from this pitch to 512 vibrations. He had heard a *second* combination tone with the interval 256:300 and with three other intervals between the

[5] W. Weber, 'Ueber die Tartini'sche Töhen,' Poggendorff's *Annal. der Physik*, XV., 1829, p. 216.

[6] Krueger, *op. cit.*, p. 191.

[7] C. Stumpf, *Tonpsychologie*, II., 1890, p. 480.

[8] W. Weber, *op. cit.*, p. 219.

[9] Le baron Blein, *Exposé de quelques expériences nouveaux sur l'acoustique*, etc., 1829.

fourth and the fifth.[10] Thomas Young had, however, long before (in 1800) made a similar discovery. He says in his report to the Royal Society on *Experiments and Enquiries Respecting Sound and Light:* "But, besides this primary harmonic (*i. e.*, Tartini's tone), a secondary note is sometimes heard when the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7:8, 5:7, or 4:5, there is a recurrence of a similar state of the joint motion, nearly at the interval of 5/15, 4/12, or 3/9 of the whole period; hence in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave."[11] It is well known that the interval 4:5 (*e. g.*, $c^2:e^2$) gives, besides 1 (c), also the combination tone 3 (g^1) very distinctly.

Stimulated by the work of Weber, the Swedish acoustician, Hällström,[12] began his study of beats and combination tones, working on the violin and organ. He became convinced that the increasing beats of two diverging tones blend together, beyond a certain limit of frequency, into a continuous tone, and therefore, first established the general rule, 'that the first combination tone is determined by the difference of the vibration numbers of the primary tones.'[13] Hällström determined four orders of combination tones corresponding to the numbers $h - t$, $2t - h$, $2(h - t)$, and $3t - 2h$, where h stands for the upper and t for the lower primary tone. The third of these he heard, says Krueger,[14] only in three or four of the minor thirds, where this tone is equal in pitch to Krueger's D_4 (fourth combination tone), and once in the major third, where it is the same as D_3. Hällström observed beats of the lower primary tone with the first combination tone $h - t$, and therefore supposed that these two tones gave rise to the second combination tone, $t - (h - t) = 2t - h$. He thus obtained different *orders* of combination tones with respect to their

[10] Krueger, *op. cit.*, p. 191.

[11] *Works of Dr. Young*, by Peacock, Vol. I., 1855, p. 84.

[12] G. G. Hällström, 'Von dem Combinationstöne,' *Pogg. Annal.*, XXIV., 1832, 438 ff.

[13] Max Meyer, 'Ueber Kombinationstöne,' *Zeit. f. Psychol.*, XI., p. 178.

[14] Krueger, *op. cit.*, p. 193.

origin. Helmholtz erroneously adopted this idea[15] though it was inconsistent with his mathematical theory, as we shall see later.

Heinrich Scheibler, a silk manufacturer of Crefeld and the inventor of a tonometer, made a more careful study of beats, from resonated tones of tuning forks. His work is reported by the school teacher, Roeber.[16] 'He first conclusively established the fact that the number of beats of the mistuned (verstimmt) prime equals exactly the difference of the double vibrations of the generating tones. The point of fusion of beats into a unitary sensation he placed at 16 per second, without taking the pitch into consideration.'[17] Scheibler based his investigation 'on the results of Hällström and on the assumption that all beats, beyond the point of fusion, go over into combination tones.'[18]

But this view had long since been expressed by de le Grange,[19] and later by Thomas Young. In his report to the Royal Society, January, 1800, Young explicitly states the theory. "The greater the difference in pitch of two sounds," he writes, "the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related they communicate the idea of a continued sound; and this is the fundamental harmonic described by Tartini."[20] This view has since been known as Young's theory.

This theory, though it later became widely accepted by acousticians, did not appeal to the physicist, G. S. Ohm. 'He had defined the tone physically as a sinus vibration.'[21]

According to this definition the ear, says Helmholtz, "*perceives pendular vibrations alone as simple tones, and resolves all other periodic motions of the air into a series of pendular*

[15] *Sensations of Tone,* p. 154 d.

[16] A. Roeber, 'Untersuchungen des Herrn Scheibler über d. sog. Schläge, Schwebungen, oder Stösse,' *Pogg. Annal.,* XXXII., 1834, p. 333 ff. and 492 ff.

[17] Krueger, *op. cit.,* p. 196. But see quotation from Meyer, above.

[18] *Ibid.,* p. 197.

[19] de le Grange, 'Nouvelles researches sur la nature et la propogation du son,' *Miscel. phil.-math. soc. priv.* Taurinensis, T. I., 1759. (I have not yet seen this article myself.)

[20] *Works of Dr. Young,* by Peacock, Vol. I., 1855, p. 84.

[21] Stumpf, *op. cit.,* p. 240; Ohm, *Pogg. Annal.,* Bd. 47 (1839), p. 513.

vibrations, hearing the series of simple tones which correspond with the simple vibrations."[22]

This is the so-called Ohm's law. It is expressed in this way by Lord Rayleigh: "It is found by experiment that, whenever according to theory a simple [*i. e.*, sinus-form, or pendular] vibration is present, the corresponding tone can be heard, but whenever the simple vibration is absent, then the tone cannot be heard. We are, therefore, justified in asserting that simple tones and vibrations of a circular type [*i. e.*, the simplest form of vibrations, sinus-vibrations] are indissolubly connected. This law was discovered by Ohm."[23]

Ohm's definition of a tone gave rise to the well-known discussion between Ohm and A. Seebeck.[24] Seebeck "was not always able to recognise upper partial tones, where Ohm's law required them to exist. In other cases where he did hear the theoretical upper partials, they were weaker than the theory required. He concluded that the definition of a simple tone as given by Ohm was too limited, and that not only pendular vibrations, but other vibrational forms, provided they were not too widely separated from the pendular, were capable of exciting in the ear the sensation of a single simple tone, which, however, had a variable quality. He consequently asserted that when a musical tone was compounded of several simple tones, part of the intensity of the upper constituent tones went to increase the intensity of the prime tone, with which it fused, and that at most a small remainder excited in the ear the sensation of an upper partial tone. He did not formulate any determinate law, assigning the vibrational forms which would give the impression of a compound tone."[25] Helmholtz took up the defense of Ohm. "The difficulty we experience in hearing upper partial tones," he writes, "is no reason for considering them to be weak; for this difficulty does not depend on their intensity but upon entirely different circumstances, which could not be properly estimated until the advances recently made in

[22] Helmholtz, *op. cit.*, p. 56.

[23] Rayleigh, *Theory of Sound*, I., 1894, p. 18.

[24] Ohm, *Pogg. Annal.*, LIX., 1843, 513 ff.; LXII., 1844, 1 ff. Seebeck, *Pogg. Annal.*, LX., 1843, 449 ff.; LXIII., 1844, 353 ff. and 368 ff.

[25] Helmholtz, *op. cit.*, pp. 58–9.

the physiology of the senses."[26] He says that for some of the best musical qualities of tone, the loudness of the first upper partials is not far inferior to that of a prime tone itself. "There is no difficulty in verifying this last fact by experiments on the tones of strings. Strike the string of a piano or monochord, and immediately touch one of its nodes for an instant with the finger; the constituent partial tones having this node will remain with unaltered loudness, and the rest will disappear."[27] In this way we can convince ourselves that the first and second upper partials are by no means weak. "For tones not produced on strings this *a posteriori* proof is not so easy to conduct, because we are not able to make the upper partials speak separately. . But even then by means of a resonator we can appreciate the intensity of these upper partials by producing the corresponding note on the same or some other instrument until its loudness, when heard through the resonator, agrees with that of the former."[28] All this, it seems to me, is an argument merely to establish the fact that in the complex wave there are physical, or objective, constituents corresponding to upper partials.

Seebeck might well agree thus far with Helmholtz. His contention is that the *ear,* unlike a physical resonator, is unable to appreciate these partial physical vibrations as distinct, *i. e.,* to analyze them, except in a very imperfect manner. More to the point in dispute, then, Helmholtz points out that when a tone is first sounded alone, and the attention fixed upon it, it can be perceived even when the lower tone, with which it had on former occasions fused, appears. "In polyphonic music proper, where each part has its own distinct melody, a principal means of clearly separating the progression of each part has always consisted in making them proceed in different rhythms and on different divisions of the bars; or where this could not be done, or was at any rate only partly possible, as in four-part chorals, it is an old rule, contrived for this purpose, to let three parts, if possible, move by simple degrees of the scale, and let the fourth leap over several. The small amount of

[26] *Ibid.,* p. 58 c.
[27] *Ibid.,* p. 58 b.
[28] *Ibid.,* p. 58 c.

alteration in the pitch makes it easier for the listener to keep the identity of the several voices distinctly in mind."[29] In the case of compound tones where all the partials start together and continue with the same relative strength, and all cease at the same time, it is little wonder that an analysis into the various constituent sensations is more difficult. In such cases, even with a trained musical ear, it requires the application of a considerable amount of attention to make the analysis. The influence of the upper partials of a compound musical tone is, moreover, by no means unfelt. "They give the compound tone a brighter and higher effect. Simple tones are dull. When they are compared with compound tones of the same pitch, we are inclined to estimate the compound as belonging to a higher Octave than the simple tones. . . . It is very easy to make a mistake of an Octave. This has happened to the most celebrated musicians and acousticians. Thus it is well known that Tartini, who was celebrated as a violinist and theoretical musician, estimated all combinational tones an Octave too high.

"The problem to be solved, then, in distinguishing the partials of a compound tone is that of analysing a given aggregate of sensations into elements which no longer admit of analysis. We are accustomed in a large number of cases where sensations of different kinds or in different parts of the body, exist simultaneously, to recognise that they are distinct as soon as they are perceived, and to direct our attention at will to any one of them separately. Thus at any moment we can be separately conscious of what we see, of what we hear, of what we feel, and distinguish what we feel in a finger or in the great toe, whether pressure or gentle touch, or warmth. So also in the field of vision. Indeed, as I shall endeavor to show in what follows, we readily distinguish them individually from each other, and that this is an innate faculty of our minds. . . .

"The matter is very different when we set to work at investigating the more musical cases of perception, and at more completely understanding the conditions under which the abovementioned distinction can or cannot be made, as is the case in the physiology of the senses. We then become aware that two

[29] *Ibid.*, p. 59 d.

different kinds or grades must be distinguished in our becoming conscious of a sensation. The lower grade of this consciousness, is that where the influence of the sensation in question makes itself felt only in the conceptions we form of external things and processes, and assists in determining them. This can take place without our needing or indeed being able to ascertain to what particular part of our sensations we owe this or that relation of our perceptions. In this case we will say that the impression of the sensation in question is *perceived synthetically*. The second and higher grade is when we immediately distinguish the sensation in question as an existing part of the sum of the sensations excited in us. We will say then that the sensation is *perceived analytically*. The two cases must be carefully distinguished from each other.

"Seebeck and Ohm are agreed that the upper partials of a musical tone are perceived synthetically. This is acknowledged by Seebeck when he admits that their action on the ear changes the force or quality of the sound examined. The dispute turns upon whether in all cases they can be perceived analytically in their individual existence; that is whether the ear when unaided by resonators or other physical auxiliaries, which themselves alter the mass of musical sound heard by the observer, can by mere direction and intensity of attention distinguish whether, and if so in what force, the Octave, the Twelfth, etc., of the prime exists in the given musical sound."[30]

In an extended argument following the sentences quoted, Helmholtz points out that with respect to *all* our senses we perceive synthetically until by accident or by consciously directed experiment and attention the contents of our perceptions are further analyzed. He concludes, therefore, that the ear *does* actually take up the separate pendular vibrations of a clang, and that by practice and special training we may become able to perceive them as individual tone sensations, whereas without such practice they are unanalyzed.

It is generally agreed that physical resonators (such, *e. g.*, as the Koenig cylindrical resonators, the Helmholtz spherical resonators, tuning forks) take up only the pendular-form

[30] *Ibid.*, pp. 62 ff.

vibrations of the air, and each resonator only those vibrations, moreover, whose periods lie very near its own natural period. The more nearly perfect the resonator, the more closely must the period of the air wave correspond to its own in order to be taken up. Tuning forks, *e. g.*, respond only to frequencies very near their own. Helmholtz suggests an instructive experiment, in this connection, to show that a piano is able to analyze the vowels of the human voice into their pendular vibrations. " Raise the dampers of a pianoforte so that all the strings can vibrate freely, then sing the vowel *a* in *father, art,* loudly to any note of the piano, directing the voice to the sounding board; the sympathetic resonance of the strings directly re-echoes the same *a*. On singing *oe* in toe, the same *oe* is re-echoed. . . . The experiment does not succeed so well if the damper is removed only from the note on which the vowels are sung. The vowel character of the echo arises from the re-echoing of those upper partial tones which characterise the vowels. These, however, will echo better and more clearly when their corresponding higher strings are free and can vibrate sympathetically. In this case, then, in the last resort, the musical effect of the resonance is compounded of the tones of several strings, and several separate partial tones combine to produce a musical tone of a peculiar quality."[31]

This experiment may also well be used to illustrate Helmholtz's view of tonal analysis in the ear, if we think of each piano string as communicating with a nerve which mediates the corresponding tone sensation. The ear, like the piano, by means of the basilar membrane fibers, takes up the various constituent pendular vibrations of any complex wave. These constituent vibrations, then, call up the corresponding sensations, which, however, are usually perceived synthetically, except in case of special training and direction of attention. The resonance theory, though first clearly stated by Helmholtz, " was a conception that flitted before the minds of Thomas Young, Johannes Müller, and others."[32]

As has already been suggested, resonance may occur in

[31] *Ibid.*, p. 61 c.
[32] John G. McKendrick, in Schaefer's *Physiology*, Vol. II., 1900, p. 1179.

different degrees. "Thus a system may show free or forced vibrations. The period of a free vibration depends on the constitution of the system itself; the vibration is made by the system when disturbed from the position of equilibrium and left to itself. A forced vibration, on the other hand, has a period determined solely by the external force acting on the system. So long as the external force acts, the forced vibration continues, but a free vibration quickly dies away. Further, a vibrating system of one degree of freedom may have the amplitude of its movements reduced by damping. Damping will soon extinguish a free vibration, and its influence is felt on a forced vibration, when there is an approach to isochronism. Now, when a forced vibration is excited in any one part of a system, all the other parts are also influenced, and a vibration of the same period is excited, whose amplitude depends on the constitution of the system as a whole.[33] If a part of the system is especially affected within a certain limit of amplitude, it is in the position of a system having one degree of freedom acted on by a given force and independent of the natural period. Resonance usually occurs when there is an approximate equality of periods between the vibrating body and the resonator. In some cases, the amplitude within which resonance is possible may be considerable; in others, very small; and much depends as regards delicacy of resonance, on the degree of damping that may be called into play. . . . Tuning-forks are susceptible of sympathetic vibration to a remarkable degree, notwitstanding the difficulty of setting their mass in motion, because they admit of a long accumulation of minute impulses; but for this reason there must be precise agreement between the pitches of the two forks. It is also observed that, if a fork is thus set agoing, it continues sounding for a considerable time."[34]

Now if the analysis of tone is effected in the cochlea on the principle of resonance, we shall expect to find in that organ a rather complex differentiation of structure. Each constituent pendular vibration must be taken up by a certain part of the

[33] Cf. Rayleigh, *Theory of Sound,* Vol. I., revised edition, 1896, p. 70.

[34] McKendrick, *op. cit.,* p. 1177. On the principle of resonance, see also Lord Rayleigh, *Theory of Sound,* Vol. I., 1896.

analytic organ. Helmholtz and the followers of the resonance principle generally have repeatedly pointed out that no other theory accounts so well for the complexity which we actually find in the cochlea. Helmholtz also shows that the ear acts differently for different periods of frequency. 'Shakes' with ten interruptions to the second can be clearly and sharply executed on several instruments throughout almost the whole scale; yet from *A* downwards, in the great and contra octaves, they sound rough and begin to fuse or run together. On these instruments, however, the interruptions can be made just as sharply at the base end of the scale as at the other end, so this fusing together cannot be due to the mechanism of the instrument. "Now since the difficulty of shaking in the base is the same for all instruments, and for individual instruments is demonstrably independent of the manner in which the tones are produced, we are forced to conclude," says Helmholtz, "that the difficulty lies in the ear. We have, then, a plain indication that the vibrating parts within the ear are not damped with sufficient force and rapidity to allow of successfully effecting such a rapid alternation of tones.

"Nay more, this fact further proves *that there must be different parts of the ear which are set in vibration by tones of different pitch and which receive the sensation of these tones.*"[85]

Professor A. M. Mayer, in 1875, published the results of a more extended and quantitative study of this same phenomenon. He interrupted the tone of a tuning fork by rotating between it and the attuned resonator a disk with a certain number of perforations. "A rubber tube led from the nipple of the resonator to one ear, while the other ear was tightly closed with a lump of bees-wax. . . . On slowly rotating the disk I perceived a series of sharply separated explosions or beats. On gradually increasing the velocity of the disk these explosions gradually approached each other; and on reaching a certain frequency in their succession they blended into a continuous smooth sensation, similar to that experienced when the disk was removed and the fork vibrated gently before the

[85] *Ibid.*, p. 143 d.

resonator."[36] After this point of blending was reached a further increase in the velocity of the disk did not change the character of the sensation. "Extreme velocities, of course produce such violent agitations at the mouth of the resonator as to render experimenting impossible."

With the aid of two competent subjects Professor Mayer obtained results which he considered more reliable than those which he had previously published in the *American Journal of Science* for October, 1874. These results are succinctly given in the following table:[37]

S	*N*	*D*		*L*
Ut_1	64	1/25	.0395 sec.	2.5
Ut_2	128	1/45	.0222 sec.	2.8
Ut_3	256	1/70	.0142 sec.	3.6
Sol_3	384	1/102	.0098 sec.	3.7
Ut_4	512	1/130	.0076 sec.	3.9
Mi_4	640	1/152	.0065 sec.	4.1
Sol_4	768	1/166	.0060 sec.	4.6
Ut_5	1024	1/180	.0055 sec.	5.6

S represents the tone used; *N*, the number of double vibrations; *D*, the durations of the residual sensation, *i. e.*, the reciprocal of *D* is the number of interruptions per second required for a continuous tone. *L* is the number of wave-lengths contained in the separate impulses into which the sound had been divided in order to produce the continuous sensation; *e. g.*, $64 \div 25 = 2.5 +$.

From his results Mayer constructs a curve illustrative of the law which he seems to have established. "From the discussion of the curve of the experiments," he says, "we find that the law connecting the pitch of a sound with the duration of its residual sensation may be expressed thus,

$$D = \frac{3.2}{N + 31} + .002,$$

in which *D* equals, in fractions of a second, the duration of the residual sonorous sensation corresponding to *N* number of vibrations per second."[38]

[36] *Philosophical Magazine*, 4th Series, LIX., 1875, p. 353.

[37] *Ibid.*, p. 355.

[38] *Ibid.*, p. 356.

Helmholtz did not hold that the ear is a *perfect* resonator; sympathetic vibration in the cochlea is not of a very high order of sensitiveness, for, unlike tuning forks, the vibration soon subsides when the impinging wave ceases to act upon it; and, furthermore, individual parts of the basilar membrane or cochlea, though vibrating most easily to the periods of their own frequency, also respond less strongly to periods *near* their own. "This sympathetic vibration is still sensible for the interval of a semitone."[39] This is of course equivalent to saying that the vibration in the cochlea, answering to external vibrations, is to a degree forced and not wholly sympathetic. On this basis, then, he meets the objection urged later by several critics,[40] that so small structures are incapable of responding sympathetically to so slow vibrations as those of the lowest audible tones.[41]

That the ear can perceive such phase variations as are represented by the beating of two neighboring tones, is explained, not from the physical form of the complex wave, but from the fact that the areas of the basilar membrane affected by the two tones respectively overlap, so that each vibration series is periodically strengthened and weakened.[42] A thin string stretched on a sounding board on which are placed two vibrating tuning forks of nearly the same pitch, may be seen to vibrate in the same fashion. A stretched membrane, somewhat resembling the drum of the ear, may be made to carry a small stiff stylus. This stylus is made to draw the vibrations of a membrane on a rotating cylinder. When this membrane, then, is set into sympathetic vibration by two tones that beat, the undulating line drawn by the stylus shows that periods of strong vibration alternate with periods of almost entire rest. Similar curves have been made by Dr. Politzer, who attached the writing stylus to the auditory bone (the columella) of a duck, and then produced a beating tone by means of two organ pipes of

[39] *Sensations of Tone*, p. 144 c.

[40] *E. g.*, Hermann, *Archiv f. d. g. Physiologie*, LIX., 1891, p. 515; Max Meyer, *Zeitschr. f. Psychol.*, XVI., 1898, p. 20.

[41] Cf. Helmholtz, *op. cit.*, p. 146 d.

[42] *Ibid.*, p. 166; cf. article on 'Ebbinghaus' Explanation of Beats,' by Bentley and Titchener, *Amer. Jour. of Psychol.*, XV., p. 66.

nearly the same pitch. This experiment shows, says Helmholtz, that even the auditory bones follow the beats of two tones. "Generally this must always be the case when the pitches of two tones struck differ so little from each other and from that of the proper tone of the sympathetic body, that the latter can be put into sensible vibration by both tones at once. Sympathetic bodies which do not damp readily, such as tuning-forks, consequently require two exciting tones which differ extraordinarily little in pitch, in order to show visible beats, and the beats must, therefore, be very slow. For bodies readily damped, as membranes, strings, etc., the difference of the exciting tones may be greater, and consequently the beats may succeed each other more rapidly.

"This holds also for the elastic terminal formations of the auditory nerve fibers. Just as we have seen that there may be visible beats of the auditory ossicles, Corti's arches[43] may also be made to beat by two tones sufficiently near in pitch to set the same Corti's arches in sympathetic vibration at the same time. If then . . . the intensity of auditory sensation in the nerve fibers involved increases and decreases with the intensity of the elastic vibrations, the strength of the sensation must also increase and diminish in the same degree as the vibrations of the corresponding elastic appendages of the nerves. In this case also the motion of Corti's arches must still be considered as compounded of the motions which the two tones would have produced if they had acted separately. According as these motions are directed in the same or in opposite directions they will reinforce or enfeeble each other by (algebraical) addition. It is not till these motions excite sensation in the nerves that any deviation occurs from the law that each of the two tones and each of the two sensations of tones subsist side by side without disturbance."[44]

Now it becomes very evident that Helmholtz could not accept the theory of Thomas Young, that beats when they become rapid enough pass over into continuous tones. As the two

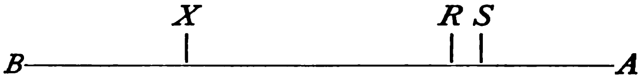
[43] While he speaks in terms of Corti's arches it is to be understood that, according to his later view, these only *mediate* the vibrations of the basilar membrane fibers. His argument, of course, applies equally well to these fibers.

[44] Helmholtz, *op. cit.*, p. 166 c.

primary tones diverge, the sections of the basilar membrane affected by these tones also separate farther and farther and finally cease altogether to overlap. The beats, consequently, dependent on this overlapping of vibrating sections, must gradually diminish in intensity as they increase in frequency, and at a certain point must 'run out.' With high tones more beats can be heard per second before this point of disappearance is reached than is the case with low tones, because high tones give more beats for a given separation, *e. g.*, a semi-tone, than do low tones. Beats, then, according to Helmholtz's view can occur only when their generating tones lie near together in pitch; but the term 'generating tones' here may mean primary tones, upper partials of the primaries, or combination tones. It is important to note that on Helmholtz's theory the *place of stimulation* in the cochlea, rather than the *manner* (*i. e.*, the frequency), is directly the determining condition of the pitch of the experienced tone.[45] It is on this very point that Ebbinghaus' view differs fundamentally from that of Helmholtz. If this important point in Helmholtz's theory is kept in mind, it is easy to understand why the view that beats go over into difference tones (Young) should be thrown out of court. The stimulation to which the first combination tone ($h - t$) answers must be much farther up the cochlea than the stimulations corresponding to the primary tones, h and t. Think what an excursion the vibration for this combination tone would have to make over the basilar membrane fibers as the primary tones diverged! It might well be granted by Helmholtz, so far as I see, that two interfering pendular vibrations in the basilar membrane could give rise to pendular vibrations of other periods. A sufficient amount of asymmetry might well be found there. But these new vibrations would not give rise to new tone sensations, unless they started up, in the liquids of the cochlea, similar vibrations which could be taken up *by the fibers of that period.* To illustrate, suppose the tones c^4 and d^4, of 2,048 and 2,304 vibrations respectively, are given and that the sections of the basilar membrane thrown into

[45] *Periodicity* of course determines the *place* of stimulation, and so indirectly the pitch; cf. *Sensations of Tone,* p. 11.

sympathetic vibration by them overlap, to a degree. Now, on account of asymmetry in the membrane, new periods of vibration might be generated, *e. g.*, 256 (2,304–2,048). Suppose the sections of these stimulations are in the region represented on the accompanying line by *R* and *S*.



The new vibration series of 256 per second would of course also, then, be in this region of the membrane, whereas *the fibers sympathetic to that frequency* are located, say, in the region *X*, and the nerves corresponding to these fibers are the only ones which, when stimulated, will mediate the *sensation* of that tone.[46]

Now, the point made above is this: unless this new vibration-frequency can communicate its period to the surrounding fluid so that it can be taken up by the sympathetic fibers at *X*, no such tone will be sensed. And such a process is improbable.

This leads us naturally to Helmholtz's *positive* contribution, a new explanation of the origin of combination tones, the principle of which we have already anticipated. He had shown why on his own theory of cochlear action combination tones cannot result from rapid beats; now he was to explain how they *can* be produced, and in this very explanation he found, as we shall see, new reasons to believe that beats do not pass over into combination tones. "In the explanation of clang analysis and the operation of Ohm's law we have had to enunciate," says Helmholtz, "and constantly apply the proposition that oscillatory motions of the air and other elastic bodies, produced by several sources of sound acting simultaneously, are always the exact sum of the individual motions producible by each source separately. This law is of extreme importance in the theory of sound, because it reduces the consideration of compound cases to those of simple ones. But it must be ob-

[46] This supposition of the generation of new frequencies in the basilar membrane is only hypothetical, and is made solely for purposes of explanation. It illustrates clearly why on Helmholtz's theory one could not suppose that beats give rise to combination tones.

served that this law holds strictly only in the case where the vibrations in all parts of the mass of air and of the sonorous elastic bodies are of *infinitesimally small dimensions;* that is to say, only when the alterations of density of the elastic bodies are so small that they may be disregarded in comparison with the whole density of the same body; and in the same way, only when the displacements of the vibrating particles vanish as compared with the dimensions of the whole elastic body. Now certainly in all practical applications of this law to sonorous bodies, the vibrations are always *very small,* and near enough to being *infinitesimally small* for this law to hold with great exactness even for the real sonorous vibrations of musical tones, and by far the greater part of their phenomena can be deduced from that law in conformity with observation. Still, however, there are certain phenomena which result from the fact that this law does *not* hold with perfect exactness for vibrations of elastic bodies, which, though almost always *very small,* are far from being *infinitesimally small.* One of these phenomena, with which we are here interested, is the occurrence of *combinational tones.*"[47] "Now it may be shown that *combinational tones must arise whenever the vibrations are so large that the square of the displacements has a sensible influence on the motions.*"[48]

As a simple example Helmholtz develops mathematically the system of waves which would result from the motion of a single heavy point under the influence of two pendular wave series.

Let m represent the mass of a heavy point able to oscillate in the direction of the axis of X, and let the force which restores it to its position of equilibrium be

$$k = ax + bx^2$$

Now if we suppose that two systems of sonorous waves act upon it with the respective forces
$f \cdot \sin pt$ and $q \cdot \sin(qt + c)$, the equation of motion becomes

$$-m \cdot \frac{d^2x}{dt^2} = ax + bx^2 + f \cdot \sin pt + q \cdot \sin(qt + c).$$

[47] *Sensations of Tone,* p. 152.
[48] *Ibid.,* Appendix XII., p. 412.

From this equation Helmholtz obtains, besides the primary tones p and q, various combination tones and upper partials. Of these $2p$, $2q$, $p - q$, and $p + q$ may be considered to be of the first order; $3p$, $3q$, $2p + q$, $2p - q$, $p + 2q$, $p - 2q$, of the second order,[49] and so on. Of those of the first order the tone $(p - q)$ will have the greatest intensity, if the intensities of the primary tones are nearly the same; "the tone $(p + q)$ will be much weaker and the tones $2p$ and $2q$ will be heard with difficulty as weak harmonic upper partial tones." The intensity of the combination tones, according to this determination, must remain proportional to the product of the intensity of the generating tones; and hence must increase more rapidly than the primary tones.

"The previous assumption [which, of course, is a purely arbitrary one] respecting the magnitude of the force called into action, namely

$$k = ax + bx^2$$

implies that when X changes its sign, k changes not merely its sign, but also its absolute value. Hence this assumption can hold only for an elastic body which is unsymmetrically related to positive and negative displacements. It is only in such that the square of the displacement can affect the motion,[50] and combinational tones of the first order arise."

As is well known, Helmholtz supposed the unsymmetrical structure of the outer drum membrane of the ear to be a most favorable condition for the generation of combination tones.[51] "But a more important circumstance, as it appears to me, when the tones are powerful," he continues,[52] "is the loose formation of the joint between the hammer and anvil. If the handle of the hammer is driven inwards by the drumskin, the anvil and stirrup must follow the motion unconditionally. But

[49] This, as Helmholtz says, is according to Hällström's nomenclature. Hällström, it will be remembered, did not explain their *origin* in this way, however. The tones $p \pm 2q$, or $2q \pm p$ when the signs are changed, are the tones actually heard. Wherever these letters are used, p stands for the higher and q for the lower tone.

[50] Helmholtz, *op. cit.*, p. 413. The above determinations, constituting appendix XII., appeared in *Pogg. Annal.*, Bd. 99, 1856.

[51] *Sensations of Tone*, p. 158 b, 413 b.

[52] *Ibid.*, p. 158 b.

this is not the case for the subsequent outward motion of the handle of the hammer, during which the teeth of the two ossicles need not catch each other. In this case the ossicles may *click*. Now I seem to hear this clicking in my own ear whenever a very strong and deep tone is brought to bear upon it, even when, for example, it is produced by a tuning-fork held between the fingers, in which there is certainly nothing that can make any click at all."

Here, then, are two means by which, according to Helmholtz, combination tones may arise from two generating tones. They may be occasioned either (1) by the unsymmetrical vibration of some structure affected by both of the primary tones, or (2) by the peculiar action between the hammer and anvil of the ear. Combination tones originating in either of these ways have usually been called *subjective,* since they are generated within the ear itself. The term, of course, is a little misleading. These tones have never been reinforced by means of resonators, and have never been made to call forth sympathetic vibrations from the most delicate and carefully attuned forks.[53]

When, however, the primary tones are generated by such instruments as the polyphonic siren or the harmonium, *which have common wind supply for the generators of the two tones,* a third cause for combination tones exists. " I will here draw attention to a third case, where combinational tones may also arise from infinitely small vibrations.[54] . . . It occurs with sirens and hamoniums. We have here two openings, periodically altering in size and with a greater pressure of air on one side than on the other. Since we are dealing only with very small differences of pressure, we may assume that the mass of the escaping air is jointly proportional to the size of the opening ω, and to the difference of pressure p, so that

$$q = c\omega p$$

[53] Cf. Helmholtz, *op. cit.,* p. 153 c; W. Preyer, *Akustische Untersuchungen,* 1879 (Synopsis by Ellis in Helmholtz, *op. cit.,* pp. 531–2); Rücker and Edser, *Philosophical Magazine,* XXXIX., 1895, pp. 341–57; K. L. Schaefer, in Nagel's *Physiologie des Menschen,* III., 1905, p. 531.

[54] Would combination tones arise if the vibrations were *infinitely* small?

where c is some constant. If we now assume for ω the simplest periodic function which expresses an alternate shutting and opening, namely

$$\omega = A \cdot (I - \sin 2\pi nt),$$

and consider p to be constant, that is, suppose ω to be so small and the influx of air so copious, that the periodical loss through the opening does not essentially alter the pressure, q will be of the form

$$q = B \cdot (I - \sin 2\pi nt)$$

where

$$B = cAp.$$

"In this case the velocity of the motion of sound at any place of the space filled with air, must have a similar form, so that only a tone with the vibrational number n can arise. But if there is a second greater opening of variable size, through which there is sufficient escape of air to render the pressure p periodically variable,[55] instead of being constant, as the air passes out through the other opening, that is, if p is of the form

$$p = P \cdot (1 - \sin 2\pi mt)$$

then q will have the form

$$\begin{aligned} q &= cAP(1 - \sin 2\pi nt) \cdot (1 - \sin 2\pi mt) \\ &= cAP[1 - \sin 2\pi nt - \sin 2\pi mt \\ &\qquad - \tfrac{1}{2} \cos 2\pi(m + n)t + \tfrac{1}{2} \cos 2\pi(m - n)t]. \end{aligned}$$

Hence, in addition to the two primary tones n and m, there will be also the tones $m + n$ and $m - n$, that is, the two combinational tones of the first order.

"In reality the equations will always be much more complicated than those selected for showing the process in its simplest form. The tone n will influence the pressure p, as well as the tone m; even the combinational tones will alter p; and finally the magnitude of the opening may not be expressible by such a simple periodic function as we have selected for ω. This will occasion not merely the tones m, n, and $m + n$, $m - n$, to be produced, but also their upper partials, and the combina-

[55] This, then, seems to require 'vibrations' that are *finite*, and not 'infinitely small' as Helmholtz says above (see note immediately preceding this).

tional tones of these upper partials, as may also be observed in experiments. The complete theory of such a case becomes extraordinarily complicated, and hence the above account of a very simple case may suffice to show the nature of the process."[56]

Acousticians are now generally agreed that there are actually such tones as Helmholtz here determines theoretically. They are called *objective* combination tones because they have a cause external to the ear and may be reinforced by means of resonators. Helmholtz assures us that he himself heard them and has not only strenghtened them with resonators but has also made thin elastic membranes respond sympathetically to them.[57] He found, though, that even in cases where the primary tones were produced by instruments with a common wind-chest, "the greater part of the force of the combinational tone is generated in the ear itself. I arranged the portvents in the instrument [the harmonium]," he says, "so that one of the two generators was supplied with air by the bellows moved below by the foot, and the second generator was blown by the reserve bellows, which was first pumped full and then cut off by drawing out the so-called expression-stop, and I then found that the combinational tones were not much weaker than for the usual arrangement. But the objective portion which the resonators reinforce was much weaker."[58]

When the two generating tones have no mechanical connection, no reinforcement with resonators is possible In such cases the combination tones seem to have no corresponding *sensible* pendular vibrations in the air. They are, therefore, called 'subjective.'

For Helmholtz, however, 'subjective' is not a good term. All combination tones are 'objective' in the sense that they have corresponding pendular vibrations *external to the analyzing mechanism in the cochlea,* whether these vibrations take origin externally to the ear altogether or in the middle ear. 'It follows from the developments already given,' he says, 'that

[56] *Ibid.*, pp. 419–20, cf. also p. 157 a.
[57] *Ibid.*, p. 153 c, 157 b; especially *Pogg. Annal.*, CIX., 1856, p. 539.
[58] *Ibid.*, p. 157 c.

we do not necessarily have to seek the cause of combination tones in the manner of sensory reception (*Empfindungsweise*) by the auditory nerve, but that with two simultaneous tones of sufficient strength there can correspond to the combination tones actual vibrations of the drum and of the ossicles of the ear. These vibrations are experienced in the usual way by the nerve apparatus. Consequently the combination tones would not have a mere subjective existence, but would be objective, even though they can originate only in the vibrating parts of the ear.'[59] In certain cases we have found that their origin is entirely external to the ear.

'In the mathematical investigation of wave movements in the air,' continues Helmholtz, 'one as a rule concerns one's self only with the terms of the equations which contain the first power of displacements (Elongationen) of the air particles and one disregards the higher powers of such displacements. If the terms which contain the second power of displacements are retained, one finds the following: (1) Every point of the air mass, in which the vibrations corresponding to the individual primary tones are strong enough, becomes a center of new secondary wave systems, which correspond to the harmonical overtones of the respective [primary] tones. (2) Every point of the air mass where the vibrations of the two given primary tones simultaneously reach sufficient magnitudes (Grösse), becomes the center of new secondary wave systems which correspond to combination tones, and from these arise both difference and summation tones of the first and of higher orders.'[60]

Thus from purely theoretical considerations Helmholtz concluded that, besides the combination tones already discovered, there exist others with vibration-numbers corresponding to $h + t$, $2t + h$, etc. These tones he named *summation* tones, in contradistinction to those expressed by $h - t$, $2t - h$, etc., which he called *difference* tones.

Though summation tones are in most cases very weak, Helmholtz succeeded in hearing them and in thus verifying his theoretical determinations. Using the polyphonic siren, he

[59] 'Ueber Combinationstöne,' *Pogg. Annal.*, CIX., 1856, p. 537.

[60] *Ibid.*, p. 538.

heard not only the *first* summation tone but also those of the second order corresponding to $2p + q$, and $2q + p$ in his equation. The following table shows the summation tones of the second order which he heard:[61]

	Interval.	Sum. tone.
2:3	$(2 \times 2 + 3)$	7
	$(2 \times 3 + 2)$	8
3:4	$(2 \times 3 + 4)$	10
5:6	$(2 \times 5 + 6)$	16
4:5	$(2 \times 5 + 4)$	14

Helmholtz thus, both theoretically and experimentally, obtains a third point to urge against the Young theory, that beats go over into combination tones. His three arguments against this theory, then, are these: (1) The theory does not explain the origin of summation tones. (2) Under certain conditions combination (both difference and summation) tones exist objectively, 'independently of the ear which would have had to gather the beats into a new tone.' (3) "This supposition [*i. e.,* Young's theory] cannot be reconciled with the law confirmed by all other experiments, that the only tones which the ear hears, correspond to pendular vibrations of the air."[62]

Before we consider the various objections that have been urged to these points, let us consider for a moment a certain inconsistency in Helmholtz's own statement of the origin of combination tones. "Multiple combinational tones [*i. e.,* those of higher orders] cannot in general be distinctly heard, except when the generating compound tones contain audible harmonic upper partials. Yet we cannot assert that the combinational tones are absent, where such partials are absent; but in that case they are so weak that the ear does not readily recognise them beside the loud generating tones and first differential. In the first place theory leads us to conclude that they do exist in a weak state, and in the next place the beats of impure intervals . . . also establish their existence. In this case we, as Hällström suggests,[63] consider the multiple com-

[61] *Pogg. Annal.,* CIX., p. 521.

[62] *Sensations of Tone,* p. 156 c; p. 167 ab. On the question of the universality of Ohm's law see below, pp. 43 ff.

[63] *Pogg. Annal.,* XXIV., p. 438.

binational tones to arise thus: the first differential tone, or *combinational tone of the first order,* by combination with the generating tones themselves, produces other differential tones, or *combinational tones of the second order;* these again produce new ones with the generators and differentials of the first order, and so on."[64]

This arrangement does not at all follow from Helmholtz's mathematical deductions given above, and is, moreover, in contradiction to well-known empirical facts to be considered later.[65] The tone $2q - p$, *e. g.*, deduced mathematically from the primary tones, p and q, under certain assumed conditions, is not formed by 'a combination of a difference tone of the first order with one of the primary tones,' according to the equation $q - (p - q) = 2q - p$; neither is it dependent upon a 'subjective' upper partial tone $2q$. Let us suppose that either one of these conditions of origin may be true and see where theoretically we shall land, even on Helmholtz's own assumption of 'transformation' due to asymmetry of the ear drum. Suppose that in the vibration of the drum to the tones p and q, the tones $2p$, $2q$, $p \pm q$ arise also in consequence of the asymmetry of that organ. Evidently neither $2q$ nor $p - q$ can react back on that *same* membrane so as to produce with one of the primary tones the combination tone $2q - p$ of the second order. Such a tone, therefore, must originate, if it does so at all, in some asymmetrical structure lying *still farther in,* where both the primary tones and the tones corresponding to $2q$ or $p - q$ will operate, in a sense, as coequals. But such a succession of origins in different structures is absurd when applied to the ear where probably only two successive structures of suitable asymmetry (the outer and inner membranes) exist. But, aside from our assumption, it seems plain that all the tones deduced by Helmholtz must spring directly from the effect of the primary tones; for $p - 2q$, or $2q - p$, *e. g.*, is just as much a product of the equation as is $2q$, or $p \pm q$.

This stand, supported by an argument quite different from

[64] Helmholtz, *op. cit.*, p. 154 d.

[65] Such facts, I mean, as second difference tones appearing much louder than the first.

the above, was taken in 1881 by R. H. M. Bosanquet in his article *On the Beats of Consonances of the Form h*: 1[66] (where '*h* is nearly some whole number'). Bosanquet shows mathematically, by means of what Ellis (who favors the 'beat-tone' view) calls some 'perhaps rather hazardous assumptions,'[67] that in the asymmetry of the drum "there are six summation-tones and six difference-tones produced by direct transformation of the primaries, when the effect of terms up to the fourth order [*i. e.*, to x_4 in the similar double integration process of Helmholtz's determination, p. 412, *Sensations of Tone*] is considered."[68] Bosanquet attempts "to show that those resultant sounds which depend on terms of higher orders can become great independently of those which depend on terms of lower orders."[69] He hopes thus to meet one of the chief objections urged against Helmholtz's theory, that the tones derived from terms of higher orders are in fact sometimes "produced with the greatest intensity when the tones derived from terms of lower orders are weak or evanescent." While it is not here maintained that Bosanquet has proved his point, the present writer is of the opinion that the true solution of this difficulty must be sought along this line; for even though the beat-tone theory, urged so eloquently by Koenig, be accepted, the difficulties are probably quite as great.

It is a question, of no less importance to the theory of combination tones than the point raised above, whether the two cases treated mathematically by Helmholtz are not *in principle* the same; that is, whether the deduction of objective combination tones from primaries generated with the siren or harmonium, *e. g.*, does not rest on the same principle ultimately as the deduction of such tones from a lack of symmetry in the forces of restitution of a heavy point vibrating under the influence of two primary tones. This question will be considered later.

Helmholtz had explained beats on the principle of *inter-*

[66] *Philosophical Magazine*, 5th Series, XI., 1881, pp. 420–36 and 492–506. Cf. on the question raised, p. 492–9.

[67] Helmholtz, *Sensations of Tone*, p. 532 d.

[68] Bosanquet, *op. cit.*, p. 497.

[69] *Ibid.*, p. 495.

ference in the basilar membrane. Such interference is possible only in case of small intervals. The beating of imperfect consonances he attributed to the presence of upper partials or of difference tones; in some cases, to both of these conditions.[70] He laid most stress on the presence of upper partial tones. Bosanquet, accepting this general point of view, early made an investigation of beats of various intervals.[71] Most of his experiments were made with tones from the enharmonic organ, in which he had a series of tones 'separated for the most part by single commas.' By means of tubes properly fitted into the ear and connecting with the resonator, he was able practically to shut out all vibrations of frequencies other than those under consideration.

After considerable practice he succeeded, as did also his friend, Mr. Parratt, '*in locating the beats with the lower tones of the intervals used.*' I quote his own words. "Suppose the mistuned octave C:c was sounding, and I examined the lower note with the resonator; sometimes it appeared loud and steady, at other times as if beating powerfully. On removing the resonator-attachment from the ear, the lower note was always heard to beat powerfully. The explanation was simple. When the nipples of the resonator-attachment fitted tightly into the ears, nothing reached the ear but the uniform vibrations of the resonator sounding C. But if there was the slightest looseness between the nipple and the passage of either ear, the second note (c) of the combination got in, and gave rise to the subjective difference tone, . . . by interference of which with the C I explain the beats on that note. *These beats are therefore subjective.*"[72]

To avoid the introduction of 'all sorts of transformations depending on the greatness of the displacements,' Bosanquet used notes of moderate strength.[73] The notes employed were

[70] *Sensations of Tone,* p. 154 a, and especially ch. 10, p. 179 ff.

[71] R. H. M. Bosanquet, 'On the Beats of Consonances of The Form h:l,' *Phil. Mag.,* 5th Series, XI., 1881, p. 420 ff.

[72] *Ibid.,* p. 431.

[73] He criticises in this connection Koenig's experiments, soon to be considered. Koenig's tones were so loud that various combination tones might be introduced through transformation of the primaries on the principle explained by Helmholtz.

examined, with and without resonators, as to the presence of harmonics. These, so far as they are objective, are readily detected with resonators. The beats of the harmonics, where they existed objectively, were also examined with resonators. After a little practice the sound of these beats became familiar enough to prevent their being confused with the beats of the low notes, and the two sets of beats could be observed independently.[74]

Already Koenig had begun his excellent experiments which were reported in a series of articles,[75] later reprinted with some slight modifications in his *Quelques Expériences d'Acoustique* (1882). Koenig proves to be Helmholtz's most able opponent, and champions the cause of the theory that beats go over into combination tones, or beat-tones, as he designates them.

Koenig used powerful tuning forks actuated by an electric current and fixed before a large adjustable copper resonator. In order to vary the pitch of the forks he had the prongs hollowed out with a drill of small diameter. These borings were made to unite with each other at the stem of the fork and to communicate with a small reservoir of mercury. By raising or lowering the mercury with delicate adjustments the pitch of the fork could be lowered or raised correspondingly. This arrangement made possible an easy and gradual change in frequency of the fork within a comparatively wide range. For the lower tone of the interval he used a fork likewise resonated but not provided with the arrangement for changing the pitch.[76] "The forks that I used with resonators," says Koenig, "had no recognisable harmonic upper partials at all. The occurrence of harmonic upper partials in tuning-forks depends not so much on the lowness of their pitch and the amplitude of their

[74] *Ibid.*, pp. 427, 428. These results agree with Stumpf's and Meyer's introspections to be considered later. From my own experience I doubt very much that the two sorts of beats are as easily distinguishable as these men suppose. Of this more will be said later.

[75] The list is given by Ellis in his 'Additions' to Helmholtz's *Sensations of Tone*, pp. 527–8.

[76] R. Koenig, *Quelques Expériences d'Acoustique*, p. 87 ff.; Zahm, *Sound and Music*, 1892, pp. 315–6.

vibrations as on the relation of the amplitude to the thickness of the prongs."[77]

According to Koenig beats are produced not only in case of small intervals, as Helmholtz's theory requires, but are also heard, with less intensity, when the intervals are large. As the primary tones diverge from unison at *C* (64) the beats gradually increase in frequency. At the place where they cease to be perceived distinctly (about 12 or 13 per sec.) they begin to produce the effect of a rolling which is constantly accelerated until the interval reaches about the fourth (22 beats per sec.). Beyond the fourth to about the fifth there is a confused rumbling (ronflement), always very strong, which begins to clear up (débrouiller) as the interval approaches the sixth. Here one again hears a simple rolling, though very rapid. This rolling diminishes somewhere between the sixth and the seventh until, when the upper tone reaches the frequency of about 116 to 118 vibrations, one is again able to count 12 and 10 separate beats. These beats gradually grow slower until at the octave they disappear. These phenomena, Koenig goes on to explain, are the results of *two series of beats*. If we represent the interval by $n:n'$, the one set of beats, the louder ones, gradually increases from O to n, as the interval diverges from unison to the octave. These Koenig calls the *inferior*, or *lower* beats. At the same time the other series is decreasing in frequency from n to O. This series constitutes the *superior*, or *upper* beats. The confusion heard near the fifth is then easily explained; for the two series of beats cross, so to speak, at the frequency of $n/2$. At this place they exactly coincide. When the frequency of the inferior beats is much less than $n/2$ one hears these beats easily; when their frequency is considerably more than $n/2$ the superior beats are heard, less easily, however.

As n' rises above the octave, or $2n$, similar phenomena are experienced though with decreasing intensity. Under favorable conditions Koenig succeeded in hearing beats with intervals

[77] Koenig, Ueber den Ursprung der Stösse u. Stösstone u. s. w., *Weid. Annal.*, XII., 1881, p. 337, quoted from Ellis' translation in Helmholtz, *op. cit.*, p. 528.

as large as 1:9 ($C:d^2$) or 1:10 ($C:e^2$). These beats are of course very feeble.[78]

Now, according to Koenig's view, these beats, when they become rapid enough, are perceived as continuous tones. These he calls *beat-tones*, or beat-notes. Designating the lower beats or beat-tone by m and the upper by m', Koenig gives these formulæ for their determination:

$$m = n' - hn$$

and

$$m' = (h - 1)n - n',$$

where h is some whole number.[79] With the exception of a few cases, to be discussed later, the tones represented by m and m' in these equations are the only *primary* beat-tones that Koenig heard. *Secondary* beat-tones may, however, arise from the primary beat-tones as these have arisen from the generating tones.[80] Only in two cases did Koenig, according to his own account, hear secondary beat-tones, with the intervals 8:11 and 8:13 of the fourth accented octave. When $h = 1$, the tones represented by m and m' correspond to Helmholtz's first and second difference tones; in cases where $h = 2$, Koenig's lower beat-tone is the same as Helmholtz's second 'order' of difference tone (theoretical) corresponding to $p - 2q$;[81] and so on. Thus all Koenig's beat-tones find duplicates in Helmholtz's theoretical (at least) combination tones. Zahm says[82] that Helmholtz's theory does not explain the upper beat-notes of Koenig. This, at any rate, is not true of Helmholtz's mathematical theory, the only real explanation that he gave of combination tones. Zahm's mistake becomes evident if Helmholtz's deduction be carried a step further.[83] We shall later

[78] Koenig, *Quelques Expériences d'Acoustique*, p. 94.

[79] The terms 'lower' and 'upper,' as will be seen, have no reference to the *relative* pitches of the two tones. The *lower* beat-tone, as a matter of fact, often lies higher in the scale than the *upper*.

[80] This is denied by Krueger who has recently made a thorough study of combination tones and beats. *Phil. Studien*, XVII., 1901, p. 306.

[81] It will be remembered that Helmholtz's mathematical determination gave as combination tones of the second order $2p \pm q$ and $p \pm 2q$. Helmholtz himself heard the summation tones $p + 2q$ and $q + 2p$, cf. *supra*, p. 23.

[82] Zahm, *op. cit.*, p. 324.

[83] See below, p. 105.

consider more in detail the matter of combination tones. Suffice it here to say that any 'beat-note' which corresponds to a combination tone, as theoretically determined, need not trouble the followers of Helmholtz, *i. e.*, so far as the mere *existence* of such tone is concerned, aside from questions of audible intensity.

It would be wrong to infer that both the upper and the lower beats, or beat-tones, are always audible at the same time. This is in fact seldom the case. "If we divide the intervals examined into groups (1) from 1:1 to 1:2, (2) from 1:2 to 1:3, (3) from 1:3 to 1:4, (4) from 1:4 to 1:5, and so on, the lower beats and beat-tones extend over little more than the lower half of each group, and the upper beats and beat-tones over little more than the upper half. For a short distance in the middle of the period both sets of beats, or both beat-notes, are audible, and these beat-notes beat with each other."[84]

By means of a method for which Koenig is indebted to MM. Lissajous and Desains, he endeavors to make clear his idea of the origin of beats and beat-tones. Of two tuning forks he makes one carry on one of its prongs a smoked glass, and the other on one of its prongs a stylus, so placed that it can record on the glass the combined movement of the two forks. The forks are then actuated by an electric current. In this way a compound curve is constructed, the form of which is shown to vary according to the vibration ratio of the interval represented by the forks.[85] Koenig supposes that the ear experiences the beats—and, when these are fast enough, the beat-tones—directly from the form of the objective waves as represented by these curves. Here are his own words: "the beats of the harmonic intervals, as well as of the unison, should be deduced directly from the composition of waves of sound, and we should assume that they arise from the periodically alternating coincidences of similar maxima of the generating tones, and of the maxima with opposite signs. The similar maxima of these harmonic intervals, as in the case of unisons, will either exactly coincide, or else there will be maxima of condensation in the

[84] Ellis, in *Sensations of Tone*, pp. 529–30.
[85] Koenig, *op. cit.*, pp. 96–7.

higher tone lying between two successive vibrations of the fundamental tone, slightly preceding one and slightly following the other; but in both cases the effect on the ear will be the same, for a beat (fluctuation) is no instantaneous phenomenon, but arises from a gradual increase and diminution of the intensity of tone."[86] According to this view not only pendular vibrations, but any periodic intensity fluctuation within certain limits, may be sensed by the ear as tone. Koenig leaves to the physiologist the explanation of the actual process by which the ear accomplishes such analysis. He has, however, put forth a creditable number of experimental facts in support of his view. We proceed to examine some of them.

Among the first experiments on this subject which Koenig performed were those on periodically interrupted sounds. For this purpose he devised large brass disks with circles of holes of two cm. in diameter. He used three such disks with 16, 24 and 32 holes respectively. These disks when rotated before a tuning fork could be made to interrupt the tone at regular intervals. The interruptions, then, could very easily (by varying the rate of rotation) be increased or decreased in frequency. In some experiments the tuning-fork, not resonated, was held some distance away from the rotating disk and the tone was conducted to the circle of apertures in the disk through a tube whose diameter equaled that of the apertures. The observer was stationed at the opposite side of the disk.

Now Koenig found that when the tone of a tuning fork was interrupted in this way, a series of beats were heard, somewhat analogous to those of interference. When these beats reached a certain frequency they gave rise to a continuous tone. Thus when the tone c^2 (512 d. vib.) was interrupted 128 times per second there was heard not only the tone c^2 but also the 'interruption' tone c (128). But this was not all. Two other tones also appeared, with frequencies equal to the sum and the difference of the interruptions and the vibration number of the given tone. In the present instance these tones were $g'(512 - 128 = 384)$ and $e^2(512 + 128 = 640)$. These

[86] Ueber den Zusammenklang Zweier Töne, *Pogg. Annal.*, CLVII., p. 186. Quoted from Ellis' translation in *Sensations of Tone*.

tones, moreover, were so loud as to 'dominate' the feeble 'interruption' tone *c*. Koenig found that on substituting high tones for the given tone of the fork and keeping the rate of rotation constant, the tone *c* became increasingly distinct. When, *e. g.*, the fork Ut_6 (*i. e.*, c^4) and Ut_7, sounding very loudly, were used, so that the ratios of interruption to frequency of the given tone were respectively 1:16 and 1:32 the 'interruption' (or intermittence) tone acquired an extraordinary force, while the variation tones from the ratio 1:16 (*i. e.*, 15 and 17) were indistinct and those of the ratio 1:32 (*i. e.*, 31 and 33) were hardly perceptible.[87]

Koenig looks upon this experiment as a demonstration that pendular vibrations are not the only forms capable of being sensed as tone.

Koenig was by no means the first one, however, to perform experiments of this kind. The use of the rotating disk was preceded by other methods bearing on the same problem. One of the earliest of these methods was the rotation of a tuning-fork. As early as 1825, E. H. and W. Weber described in their *Wellenlehre* such an experiment: "If a tuning fork is put into a lathe so that it can be rotated about the longitudinal axis of its stem, it is found that the fork ceases to sound when a certain rate of rotation is reached, but that the tone reappears if the lathe is suddenly stopped. This is not to be explained by supposing that the noise of the lathe drowned the fork, for if one brings the end of a cylindrical tube close to the prongs of the fork and puts the other end to the ear, one is convinced that the rotation does not destroy the vibration of the fork, but prevents its transmission to the ear. We can give no explanation to this remarkable phenomenon."[88]

[87] *Quelques Expériences d'Acoustique*, pp. 138–9. If a tone *n* is interrupted *m* times, there result tones corresponding to n, m, $n - m$, and $n + m$. The tone corresponding to *m* is known as the 'interruption tone.' This name probably comes from Stefan (1866); cf. Bentley and Sabine, *Am. Jour. of Psych.*, XVI., note, p. 486. It is also called an intermittence tone. The tones $n - m$ and $n + m$ are known as 'variation tones' (from Radau), cf. p. 36, below.

[88] Quoted by W. Beetz, Über die Töne rotirender Stimmgabeln; *Pogg. Annal.*, CXXVIII, 1866, p. 490. Beetz's article was translated into English by G. C. Foster, *Phil. Mag.*, 4th Series, XXXII., 1866, 534 ff. I avail myself here of the statement by Bentley and Sabine of these early experiments, cf. *Amer. Jour. of Psychol.*, XVI., 487 ff.

In 1844 August Seebeck[89] theoretically investigates the matter of a tone m fluctuating periodically n times per second. He finds from mathematical deductions, that, according to Ohm's definition of a tone there should result from such fluctuation not only the tone m, but also tones corresponding to $m \pm n$, $m \pm 2n$, $m \pm 3n$, etc.[90] Seebeck did not follow up the matter experimentally. This, however, was done by Helmholtz who used for the purpose his double siren.

In 1863 Helmholtz describes his experiment in the first edition of his *Tonempfindungen.*[91] I quote from the translation of Ellis of a later edition: "The lower box of my double siren vibrates strongly in sympathy with the fork a^1 when it is held before the lower opening, and the holes are all covered, but not when the holes are open. On putting the disk of the siren in rotation so that the holes are alternately opened and covered, the resonance of the tuning-fork varies periodically. If n is the vibrational number of the fork, and m the number of times that a single hole in the box is opened, the strength of the resonance will be a periodic function of the time, and consequently in the simplest case equal to $1 - \sin 2mt$.

"Hence the vibrational motion of the air will be of the form

$$(1 - \sin 2\pi mt) \cdot \sin 2\pi nt = \sin 2\pi nt + \tfrac{1}{2} \cos 2\pi(m+n)t - \tfrac{1}{2} \cos 2\pi(m-n)t;$$

and consequently we hear the tones $m+n$, and $m-n$ or $n-m$. If the siren is rotated slowly, m will be very small, and these tones being all nearly the same, will beat. On rotating the disk rapidly, the ear separates them."[92]

In the meantime W. Beetz had been experimenting with the rotating tuning-fork. In 1851 he reported to the Physical Society at Berlin the results of a repetition of the Weber experiment mentioned above.[93]

In this experiment he had arrived at a result different

[89] 'Ueber die Definition des Tones,' *Pogg. Annal.*, LXIII., 1844, 353 ff.

[90] *Ibid.*, p. 366.

[91] *Lehre von den Tonempfindungen,* 1863, p. 597, cited by K. L. Schaefer in Nagel's *Physiologie,* III., 1905, p. 532.

[92] *Sensations of Tone,* p. 420.

[93] *Supra,* p. 32.

from that obtained by the Webers. For him the tone of the tuning-fork did not disappear; it only became weaker. "I distinctly heard at the same time," he says, "a higher tone as well as a series of beats which coincided in number with the number of half-revolutions of the tuning fork."[94] W. Weber, in a letter to Beetz, suggested 'that the reason why the higher note had not been perceptible in their [the Webers'] experiments was that they had employed a slower and more noisy lathe.' A satisfactory explanation was still wanting.

"Later Beetz again took up the experiment,[95] using two forks of 512 and 1024 vibrations per second. When these forks were rotated about twelve times per second, Beetz found that the pitch of the lower fork was raised about three-fourths of a tone, and the higher about a half tone. He heard again the beats, two for every revolution of the fork. The phenomenon, he holds, is not to be connected with the transmission of the vibrations of the fork to the air, for one hears the rise in the pitch just as well, or better, when one lays one's head on the lathe and stops one's ears entirely."[96]

"The phenomenon," says Beetz, "is thus entirely objective, and consists in a real increase of the rate of vibration of the fork. It is in fact only another form of Foucault's pendular-experiment. The vibrations tend to continue in the same plane as that in which they were produced; they are thus, as it were, transmitted to a thicker bar, and so produce a higher tone. The amplitude of the vibrations at the same time becomes smaller; gradually, however, it increases again, and reaches a minimum [Query, a maximum?—Trans. in a footnote] every time that the fork returns to its original position, or to one differing from it by 180°. It is thus that the beats are produced. . . . If the fork turns only slowly, the plane of vibration turns with it, and in this case the fundamental tone is heard above without beats; on turning more quickly, objective beats soon arise, and the tone

[94] *Phil. Mag.*, 4th Series, XXXII., 1866, p. 535.

[95] Über die Töne rotirender Stimmgabeln, *Pogg. Annal.*, CXXVIII., 1866, 490 ff. G. C. Foster translated this article into English, *Phil. Mag.*, 4th Series, XXXII., 534 ff.

[96] Quoted from Bentley and Sabine, *Am. Jour. of Psychol.*, XVI., p. 487; cf. Beetz, *Phil. Mag.*, 4th Series, XXXII., pp. 335–6.

rises at the same time, but never to an extent corresponding to the vibrations of a rod whose thickness is equal to the longer cross section of the prongs of the fork."[97] Later Beetz explains[98] that he discovered an error in his experiments which renders this explanation impossible. Furthermore, he has discovered certain lower tones which are apparently *not* objective and are not to be accounted for on the theory of the Foucault pendulum. He, therefore, attempts an explanation on the basis of the change in pitch of a moving source of sound. "The tone qualities present with a revolving fork could very well be explained in this way, but Beetz was entirely unable to make out any quantitative correspondence. The observed intervals were much too large. . . .[99]

"While Beetz was performing these experiments, J. Stefan was also carrying on investigations of a similar nature. He found that, if a vibrating plate were rotated before the ear, the characteristic tone of the plate disappeared and was replaced by two tones, the one higher, the other lower, than the primary. The higher is usually the stronger of the two, and the primary tone is sometimes audible along with the lower and higher tones. The same phenomenon is heard with a rotating tuning fork. The phenomenon to be explained, according to Stefan, is the effect upon the ear of a tone of periodically varying intensity. The movement which a tone of constant intensity produces in a body vibrating in sympathy with it can be expressed in the formula

$$a \sin 2\pi n(t + \theta)$$

where $n =$ vibration rate, t a variable, and θ a constant time, and a the amplitude of vibration. If the intensity varies periodically, a becomes a periodic function of t and in the simplest case can be expressed as

$$a \sin 2\pi n'(t + \theta');$$

n' being the number of intensity changes in a unit of time. a is

[97] Quoted from Foster's translation of Beetz, *op. cit.*, p. 536.

[98] 'Über die Töne rotirender Stimmgabeln,' *Pogg. Annal.*, CXXX., 1867, pp. 313–7.

[99] Bentley and Sabine, *op. cit.*, pp. 487–8.

then a constant quantity. If now one substitute this formula for a in the first, one gets for the excursion of the sympathetically vibrating body

$$a \sin 2\pi n'(t + \theta') \sin 2\pi n(t + \theta)$$

or

$$a/2 \cos \pi(n - n')(t + \theta_1) - a/2 \cos 2\pi(n + n')(t + \theta_2).$$

But each of these expressions represents a single pendular vibration, the one having a vibration rate of $n - n'$, and the other a rate of $n + n'$. By actual observation, Stefan found that his lower and higher tones corresponded in pitch to the demands of this explanation."[100]

In a second article which appears in the same periodical,[101] Stefan describes some experiments in which he interrupted the tone of a tuning-fork by the method already described,[102] which Koenig used later. We learn from this report that Professor Ernst Mach[103] had, at about the same time, also employed this method. Both investigators heard the tones corresponding to $m - n$ and $m + n$ of Helmholtz's equation, which Radau[104] had in the meantime determined theoretically. Radau calls these tones *variation tones.*

Beetz, stimulated by the results and the hypothesis of Stefan and Radau, takes up again the experiments with a view to testing this hypothesis.[105] "Finding the hypothesis well borne out with rotating plates, he turned again to rotating forks to determine whether they too gave the tones to be expected from Stefan's formula. He found that the lower tone as observed corresponded approximately to the calculated tone. The higher tone, however, was always much higher than the theory required. The difference between the observed and calculated

[100] Bentley and Sabine, *op. cit.,* p. 488; cf. also 'Über einen akustischen Versuch,' *Sitzungsber. d. kais. Akad. d. Wiss. zu Wien, Math.-naturwiss. Kl.,* LIII., Abt. 2, 1866, 696 ff.

[101] *Ibid.,* LIV., Abt. 2, 1866, 597 ff.

[102] *Supra,* p. 31.

[103] 'Ueber die Aenderung des Tones und der Farbe durch Bewegung,' *Pogg. Annal.,* CXII., 1861, 58 ff.

[104] *Moniteur Scientifique,* 1865, 430 ff.

[105] 'Ueber den Einfluss der Bewegung der Tonquelle auf die Tonhöhe,' *Pogg. Annal.,* CXXX., 1867, 587 ff.

values became very large with rapid rates of rotation. Beetz found, however, that the difference became trifling when he took his observations with a resonator having an opening 5 mm. instead of 25 mm. in diameter. With such a resonator the observed values coincided very closely with the values computed by Stefan's formula. Beetz used these forks, $c^1 = 256$, $a^1 = 440$, $c^2 = 512$, and three rates, 6.5, 13 and 19.5 revolutions per second. He took, also, some observations for two very low forks, 64 and 77 vibrations. In almost every case, Beetz's observed values are larger than the calculated values. In this last paper, Beetz accepted Stefan's and Radau's explanation of the phenomenon."[106]

All these studies have been made by men interested in physics, primarily, and they have treated these *variation tones* as actually existing *objectively as pendular vibrations.* Stefan and Beetz had actually used resonators in determining their pitch. Moreover, while explanations of the phenomenon at first differed considerably, or were entirely withheld, one cannot help being struck with the steady advance toward uniformity of opinion as theoretical determinations go hand in hand with the accumulation of empirical data. The outcome of the whole process is a brilliant achievement. Yet in the face of all this, Koenig, also a physicist, considered the *interruption,* or intermittence tone, an objection to the resonance hypothesis, without, apparently, testing for its objectivity![107]

In 1875 Professor A. M. Mayer, engaged on another problem, but using the same sort of interruption apparatus as Koenig employed, incidentally noticed this phenomenon of resultant tones and describes it practically as did Koenig. "When the disk is stationary, with one of its openings opposite the mouth of the resonator, it is evident that the ear will experience a simple sonorous sensation when a tuning-fork is brought near the mouth of the resonator. On revolving the perforated disk, two additional or secondary tones appear,—one slightly above,

[106] Bentley and Sabine, *op. cit.,* p. 489.

[107] None of these formulæ, it is true, show a vibration corresponding to this tone *m,* whose frequency number is equal to the number of interruptions; but we shall see that two explanations may be given of its origin, neither of which is contradictory to the resonance hypothesis.

the other slightly below the pitch of the fork. An increase in velocity of rotation causes the two secondary sounds to diverge yet further from the note of the beating fork, until, on reaching a certain velocity, the two secondary sounds become separated from each other by a major sixth, while at the same moment a resultant sound appears, formed by a union of the fork with the upper and lower of the secondary sounds. The resultant is the lower second octave of the note given by the fork.[108] On further increasing the velocity of rotation of the disk, the two secondary sounds and the resultant disappear, and the ear experiences only the sensation of the simple sound produced by the fork, whose beats at this stage of the experiment have blended into a smooth continuous sensation."[109]

Mayer's statement, at the conclusion of his article, that the beats from this rotating disk in the interruption of a continuous sound are like those due to the interference of two tones nearly equal in pitch, calls out from Lord Rayleigh the following: "The difference between the two kinds of beats is considerable. If there are two vibrations of equal amplitude and slightly differing in frequencies, represented by $\cos 2\pi n_1 t$ and $\cos 2\pi n_2 t$, the resultant may be expressed by

$$2 \cos \pi (n_1 - n_2) t \cos \pi (n_1 + n_2) t,$$

and may be regarded as a vibration of frequency $\frac{1}{2}(n_1 + n_2)$, and of amplitude $2 \cos \pi (n_1 - n_2) t$. Hence, in passing through zero, the amplitude changes sign, which is equivalent to a *change of phase of* 180°, if the amplitude be regarded as always positive. This change of phase is readily detected by measurements in drawings traced by machines for compounding observations. If a force of the above character act upon a system whose natural frequency is $\frac{1}{2}(n_1 + n_2)$, the effect produced is comparatively small. If the system start from rest, the successive impulses coöperate at first, but after a time the later impulses begin to destroy the effect of former ones. The greatest response is given to forces of frequency n_1 and n_2, and not of a force of frequency $\frac{1}{2}(n_1 + n_2)$.

[108] This is probably the so-called interruption tone. He seems to regard this tone as a difference tone. Cf. Schaefer's view, p. 86, below.

[109] *Phil. Mag.*, 4th Series, LIX., 1876, p. 358.

"On the other hand, where a single vibration is rendered intermittent by the periodic interposition of an obstacle, there is no such change of phase in consecutive revivals. If a force of this character act upon an isochronous system, the effect is indeed less than if there were no intermittence; but as all the impulses operate in the same sense without any antagonism, the response is powerful. An intermittent vibration or force may be represented by

$$2(1 + \cos 2\pi mt) \cos 2\pi nt,$$

in which n is the frequency of the vibration, and m the frequency of the intermittence. The amplitude is always positive, and varies between the values 0 and 4. By ordinary trigonometrical transformation the above expression may be put into the form

$$2 \cos 2\pi nt + \cos 2\pi(n + m)t + \cos 2\pi(n - m)t;$$

which shows that the intermittent vibration is equivalent to *three* simple vibrations of frequencies n, $n - m$, and $n + m$. This is the explanation of the secondary sounds observed by Mayer."[110]

From tones that are periodically interrupted Koenig went to periodically variable tones. In this experiment Koenig hoped by a sort of synthetic process to produce beats similar to those due to interference of neighboring tones and by increasing their frequency to prove that they produce a continuous tone. He used a large disk with seven circles of 192 holes each. The holes of each circle varied periodically in diameter from 1 to 6 mm. The seven circles of holes were arranged to vary periodically from the outside inward 12, 16, 24, 32, 48, 64 and 96 times respectively. In blowing against these circles through a tube 6 mm. in diameter when the disk was in rotation, Koenig heard not only the tone corresponding to 192—the number of holes to the revolution—but also, when the rotation was fast enough, the one in each case corresponding to the number of variations in diameter—*i. e.,* 12, 16, 24, etc., as the case may be. When the rotation was slow he heard a beating; as it

[110] *Phil. Mag.,* IX., 1880, pp. 278–9; cf. also Rayleigh, *Theory of Sound,* Vol. I., 1894, pp. 71–2.

increased, the beats became more and more rapid until they finally blended into a second pure tone.[111]

Zahm says that 'the foregoing experiment would seem to be conclusive as to the true nature of beats and beat-tones.'[112] Koenig was not so sure of this, however. He recognized that if the complex vibration thus produced were actually like that resulting from two interfering tones, we should expect to hear in this case not only the 'beat-tone' but two other tones corresponding to two primary tones which would produce similar beating. To quote: 'In short, if a series of 96 isochronous impulses of which the intensity increases and decreases 16 times represent exactly two tones which give 16 beats, one ought to hear the two primary tones in question (which would here be the tones 88 and 104, forming the interval 11:13); but these tones are never produced. The reason for this difference between the beats and the separate isochronous impulses of periodic intensity ought to be sought in the fact that the compound of two tones a and b near unison is equal to a tone of medium pitch $(a+b)/2$ of which the intensity not only increases and diminishes periodically but undergoes a *change of sign* once during each beat, as the formula

$$\sin a + \sin b = 2 \cos \frac{a-b}{2} \cdot \sin \frac{a+b}{2}$$

shows, where $\cos (a-b)/2$ represents the periodic intensity of the tone $(a+b)/2$.'[113]

When Mr. Spottiswoode reported Koenig's experiment,[114] Lord Rayleigh did not consider it at all convincing against Helmholtz's theory, *i. e.*, that beats do *not* generate combination tones. He made the same distinction between these 'beats' and those due to interference that Koenig did, consequently he could not see how beats of *interference*, with this change of phase,[115] could generate a tone sensation. Bosanquet raised the

[111] Koenig, *op. cit.*, pp. 140–142, and *Pogg. Annal.*, CLVII., 1876, pp. 177 ff.; cf. also Zahm, *op. cit.*, pp. 334–5.

[112] Zahm, *op. cit.*, p. 335.

[113] Koenig, *op. cit.*, p. 143.

[114] *Proc. Mus. Ass.*, 1878–9, p. 128.

[115] *Supra*, p. 38.

same objection, and developed it more fully in his article referred to above.[116] He shows, by a mathematical expression similar to that developed by Koenig above, that the 'resultant displacement'[117] should produce a tone "having oscillations of intensity whose frequency is defined by a pendulum-vibration of frequency equal to half the difference of the frequencies of the primaries. This," he says, "is what is actually heard in case of two notes less than two commas apart." If this held for widely separated intervals "the primary notes would not be heard at all, and the note that would be heard would have the arithmetic mean of the frequencies of the primaries.

"*E. g.*, in the case of a fifth (4:6) the note heard would be the major third (5), which would beat very rapidly. . . . But as a matter of fact, the note 5 is not heard at all in the above case." Again, 'supposing that in some unexplained way the beats whose speed is $(p - q)/2$ [*i. e.*, half the difference of the frequencies of the primaries] . . . gave rise to a note, as supposed by Koenig. Then the speed of that note does not agree with that required for Koenig's first beat-note, which has the same speed as Helmholtz's difference-tone, or $(p - q)$.'

Koenig seems fully to have appreciated the force of these objections, for he immediately takes up experiments to meet them.[118] "If two tones of 80 and 96 d. vib. are sounded together," he says, "they generate a tone of $\frac{1}{2}(80 + 96) = 88$ vibrations with an intensity increasing and diminishing 16 times, and at each passage from one beat to another there is a change of sign, so that the maximum of compression of the first vibration of the following beat is half a vibration behind the maximum compression of the last vibration of the preceding beat."[119] To meet this case he performed two experiments.

[116] *Phil. Mag.*, 5th Series, XI., 421–3.

[117] Where the two vibrations $\cos pt$ and $\cos(pt - \epsilon)$, having equal amplitudes $(= 1)$, are combined on the same receptive mechanism,

$$2 \cos \frac{(p+q)t - \epsilon}{2} \cdot \frac{(p-q)t + \epsilon}{2}$$

is the 'resultant displacement.'

[118] Koenig, 'Ueber die Zusammenklang zweier Töne,' *Pogg. Annal.*, CLVII., 1876, pp. 177–237.

[119] *Ibid.*, p. 252, quoted from Ellis' translation in *Sensation of Tone*, p. 534.

In the first he divided a circle on a large disk into 176 equal parts, numbering the parts in their order. In the five points 1, 3, 5, 7, 9, he drilled five holes, gradually increasing in diameter to 5 mm. and then diminishing. He did likewise in the points 12, 14, 16, 18 and 20; and in the points 23, 25, 27, 29 and 31; and so on. "When such a disk was blown upon through a pipe with the diameter of the largest opening, in addition to the tone 88 and the very powerful tones of the period 16 both of the tones 80 and 96 could be heard, but they were very weak, and, on account of the roughness of the deep tone, difficult to observe." 'In this case the phase was the same throughout,' says Ellis.[120] Koenig, too, apparently considered it so.

To imitate the *change of phase* Koenig divided each of two concentric circles on a disk into 88 parts and disposed the holes which were to represent the successive beats alternately on each circle. As 88 holes and 16 periods give 5½ holes to each period, Koenig 'took two periods together, and pierced on the first circle the divisional points 1, 2, 3, 4, 5, 6 and on the second 6, 7, 8, 9, 10, 11, then again on the first 12, 13, 14, 15, 16, 17 and on the second 17, 18, 19, 20, 21, 22, and so forth.'[121] The size of the holes, in this case also, gradually increased and diminished to represent beats. 'When these circles of holes were blown upon at the same time through two pipes of the diameter of the largest opening, and placed on the same radius, one circle from above and the other circle from below, then at each revolution of the disk there were created 88 isochronous impulses, varying 16 times in intensity, which changed sign on each transmission from one period of intensity to the other. In this experiment the two tones 80 and 96 were more distinct than in the first experiment, where the circles of holes were blown upon from one side only.'[122]

In reply to Bosanquet's objection,[123] that Koenig's beat-note

[120] Lord Rayleigh says: 'In passing through zero, the amplitude changes sign, which is equivalent to a *change of phase of* 180°, if the amplitude be always regarded as positive,' *supra*, p. 38.

[121] Cf. Fig. 41, *Quelques Expériences d'Acoustique*, p. 144.

[122] *Pogg. Annal.*, CLVII., pp. 232–3; cf. also *Quelques Expériences d'Acoustique* (where the two kinds of disks are shown in Figs. 40 and 41), pp. 144–5.

[123] *Supra*, pp. 40, 41.

ought not to agree in pitch with Helmholtz's first difference tone, Koenig in his later French edition[124] says: 'the change of phase of the separate vibrations of a variable amplitude, forming the beats, does not cause these maxima of intensity to be produced in contrary directions. Besides, these maxima remain isochronous, and consequently fulfill the conditions under which primary impulses are combined to form sounds. The only influence which the change of phase in question exerts on the disposition of the waves consists in the fact that these maxima of intensity do not stand apart by a whole number of complete vibrations $((a + b)/2)$, but by an odd number of half-vibrations. . . . Notwithstanding the change of phase, the beat-note must always have the same frequency as the beats.'

Koenig has further studied the influence of phase separate from the phenomena of beats. For this he employed his well-known wave-siren. "He first drew to scale the curves obtained by compounding partials up to the tenth number of the series; and he modified the curves so that they were compounded first with zero difference of phase, then with all the upper members moved one quarter, then with a difference of half a wave, and lastly with a difference of three quarters. The sounds of all these curves, according to Helmholtz, should be exactly alike, although they differed in form and position." These curves were reproduced accurately in the proper size by photography, inverted so that the high parts became low and the low high and then were cut out on the circumference of metallic cylindrical hoops. "These hoops were then mounted on an axis and rotated rapidly. Against these toothed edges (or edges showing sinusoidal curves) air was blown under pressure through narrow slits as the curves passed in front of the slits; and thus sounds varying in phase from a quarter to three-fourths of the wave-length were obtained. It was found that they *varied in quality*."[125] Koenig's own statement of the results follows: "The composition of a number of harmonic

[124] *Quelques Expériences d'Acoustique,* p. 143, note.

[125] McKendrick, in Schaefer's *Physiology,* Vol. 2 (1900), p. 1176; cf. Koenig, Bemerkungen über die Klangfarbe, *Wied. Annal.,* XIV., 1881, pp. 369–93; and especially *Quelques Expériences d'Acoustique,* pp. 222 ff., where there are illustrative figures of the curves and apparatus.

tones, including both the evenly and unevenly numbered partials, generates in all cases, quite independently of the relative intensity of these tones, the strongest and acutest quality tone for the ¼ difference of phase, while the difference 0 and ½ lie between the others, both as regards intensity and acuteness.

"When unevenly numbered partials only are compounded, the differences of phase ¼ and ¾ give the same quality of tone, as do also the differences 0 and ½; but the former is stronger and acuter than the latter.

"Hence, although the quality of tone principally depends on the number and relative intensity of the harmonic tones compounded, the influence of difference of tone [phase?] is not by any means so insignificant as to be entirely negligible. We may say, in general terms, that the differences in the number and relative intensity of the harmonic tones compounded produces those differences in the quality of tone which are remarked in musical instruments of different families, or in the human voice uttering different vowels. But the alteration of phase between these harmonic tones can excite at least such differences of quality of tone as are observed in musical instruments of the same family, or in different voices singing the same vowel."[126]

"A ready appreciation of such minor differences," says Lord Rayleigh, "requires a series of notes, upon which a melody can be executed, and they may escape observation when only a single note is available. To me it appears that these results are in harmony with the view that would ascribe the departure from Ohm's law, involved in any recognition of phase relations, to secondary causes."[127] McKendrick thinks, from the results of this experiment, that the influence of phase is, however, not so absolutely negative as Helmholtz supposed.[128] Undoubtedly Koenig's method, ingenious as it is, would not allow of the reproduction of the tone exactly corresponding to the compound wave form. Any conclusion, therefore, drawn from the experiment described must take this fact into consideration.

Hermann, who adopts the view that beats run into com-

[126] *Wied. Annal.*, p. 391, trans. by Ellis in Helmholtz, *op. cit.*, p. 537.

[127] Rayleigh, *op. cit.*, Vol. II., p. 469.

[128] Schaefer's *Physiology*, Vol. II., p. 1176.

bination tones or beat-tones, holds that Koenig's experiment with the wave siren is inadequate as a proof that phase change of upper partials has an effect on the quality of the tone. The compressibility of the air, he holds, makes it altogether improbable that the air vibrations take the exact form of the curve on the wave siren.[129] He considers the siren, though a useful apparatus for other experiments, entirely unfit for use on the phase problem. He, therefore, took up experiments with Edison's phonograph,[130] and came to the conclusion that not the phase change in itself but the effect that is produced by such shift of upper partials on the position of the maxima in the compound curve and on the amplitude of vibration accounts for the change in the sound.[131] Hermann had already concluded that, in order to explain certain phenomena of beats and combination-tones, we must '*ascribe to the ear the power of answering with a tone sensation to every sort of periodicity within certain limits.*'[132] According to this supposition the question of phase needs to be formulated differently. It is best to let the word phase drop altogether and simply to say that the tone is conditioned not only by the amplitude of the partial vibrations but also, and chiefly, by the form (Gestalt) of the resulting curve. Hermann finds that, other things being equal, *the tone is always 'sharpest' when the change from maximum to minimum* (or vice versa) *is most abrupt.* So, at least, I understand him.[133] In any case, Helmholtz's theory seems to be insufficient on this point.

To recur then to the experiments of Koenig, we may well say that the results he obtained were in no way conclusive. That there is such a thing as an 'interruption' tone is by no means established. Physical theory shows that the so-called variation tones exist objectively both when they are produced

[129] Pflüger's *Archiv f. d. gesammte Physiol.*, LVI., 1894, p. 474.

[130] *Ibid.*, LIII., 1892, 8 f.; also LVI., 476 ff.

[131] *Ibid.*, LVI., p. 484.

[132] *Ibid.*, LIX., 1891, p. 514.

[133] *Ibid.*, LVI., p. 473. "*Der Schall ist unter sonst gleichen Umständen am shärfsten, wenn die Spaltöffnung durch die Curve so plötzlich als möglich verdeckt oder freigelegt wird, und zwar wirkt plötzliche Verdeckung weit schärfer als plötzliche Oeffnung.*"

as a consequence of periodic intermittence of a constant tone, and when they result from such intensity changes as are produced by the rotation of a sounding tuning-fork. Indeed both of these cases, as physical theory shows, may be treated together. From the experimental results of Koenig it is not so certain that the case of periodic intensity fluctuation produced by perforations of varying diameter may be classed with the other cases of periodic intensity changes or intermittence; for in this case the variation tones are not mentioned, and the 'interruption' tone is very prominent.[184] Koenig has, however, by no means proved that beats of interference of neighboring tones produce beat-tones, even though it be granted that the beats of intensity variation are capable, when their frequency is sufficient, of producing a continued tone-sensation. Mathematical theory shows that in the case of beats from interference there is a *change of phase*. Even though Koenig, by an ingenious method, has endeavored to imitate this change of phase in case of the beats from intensity fluctuation, and under those conditions has still obtained the so-called interruption tone, it is not at all certain that he actually obtained the conditions that he sought. It is still an open question whether the ear perceives intensity fluctuations occurring with changes of phase.[185]

[184] But cf. K. L. Schaefer's experiments reported below, p. 88.

[185] Since the above account was written some recent articles on the perception of phase-difference have appeared in the *Philosophical Magazine*. Lord Rayleigh in No. 74 (February, 1907) reports experiments in which two tones of nearly equal pitch were conducted separately one to each ear of the subjects. Under such conditions the location of the source of sound seemed to fluctuate from right to left, always being on the side of the more advanced phase which of course shifted. Rayleigh also (cf. No. 75, for March) transmitted tones to the subjects' ears by means of two telephones, changing the phase by means of a commutator arrangement. The results agree with those of the first series of experiments. Lord Rayleigh concludes that "we are able to take account of phase-difference at the two ears."

In No. 76 (April, 1907) of the same magazine, L. T. Moore and H. S. Fry report experiments bearing on the same question. They transmitted one tone to the two ears by means of a Y-tube. When one of the branch tubes was made slightly longer than the other so that the wave reached the corresponding ear in later phase than in the other ear, the sound was located on the side of the short tube. These results, therefore, agree with those of Lord Rayleigh.

These results are, of course, inexplicable on the Helmholtzian theory, as on most others. Lord Rayleigh thinks that they uphold Rutherford's theory. This

And lastly, the experiment on the wave siren to ascertain whether differences of phase affect the nature of the tone, though it seems to point slightly to the affirmative, is not beyond question as to its validity. But the results obtained seem to indicate that phase differences are not so negative in their effect as Helmholtz supposed.

In 1881 Koenig again tried to show experimentally that beats when frequent enough can generate sensations of tone. He now employed his wave siren, already described. The simplest method employed was to draw out, on a large scale, two harmonic curves and then to construct from them the resultant compound curve by well-known methods of measurement. By constructing these curves on a large scale, Koenig was able to reduce errors. The drawing of the compound curve was then reduced by photography to the required dimensions. The curve thus constructed was then inverted and carefully reproduced on the edge of a metallic cylindrical hoop which was so mounted on an axis that it could be rotated rapidly. The reason for the inversion of the curve is that the elevations representing greater intensities would now give lesser intensities, on account of shutting off the current more than the depression would do. The instrument as used by Koenig had four such tooth-edged hoops, so that he could examine a number of intervals conveniently and compare results.[186]

When such a toothed rim is rotated before a slit fixed over it in the proper direction, and of a length at least equal to the greatest height of the curve, the slit will be periodically shortened and lengthened according to the law of the curve; and if wind is blown through the slit, a motion in the air must be generated corresponding to the same law. And this motion must be precisely the same as that produced by the simultaneous sounding of two really simple tones without any admixture of upper partials. The advantage of this arrangement, then,

theory supposes that the place of tone analysis is in the cortex and not in the ear, and really *explains* nothing. Certainly in these experiments the intensity difference and the possibility of bone conduction were not sufficiently controlled, and it is unsafe to place too much faith in the results until further experiments along the same line shall have been made.

[186] See Fig. 43, *Quelques Expériences d'Acoustique*, p. 160.

is that we know precisely what tones are acting and that they are undoubtedly simple.[137] So reasoned Koenig. He constructed disks and bands, as described above, giving the intervals 8:9, 8:10, 8:11, etc., up to 8:24. He thus describes his results: "The disks[138] for different intervals, when the rotation was slow gave beats, and when it was more rapid, beat-notes, exactly corresponding to those observed when two tuning-forks are sounded together. Thus the major second 8:9 produced the lower beat-note 1; the major seventh 8:15, the upper beat-note 1; the disturbed twelfth 8:23, the upper beat-note of the second period, which is again equal to 1, loudly and distinctly. In the same way the ratios 8:11 and 8:13 gave quite distinctly and at the same time the upper and lower beat-notes 3 and 5 for the first, and 5 and 3 for the second."[139]

"The evidence, then, that beats may coalesce and blend into a continuous tone is conclusive," says Zahm. "The more the movement of the air excited by impulses of any kind, differs from a simple pendular motion, the more these impulses will be separately distinguishable, and the less the sound due to their coalescence will be perceptible. On the other hand, the more nearly the periodic motion of the air approaches to a pendular motion, the less distinct will the separate impulses become, and the stronger the resultant tone. Wherefore, with the almost absolute pendular motion of tuning-forks, the separate impulses beyond 32 and 36 cease to be perceived, and the sound resulting therefrom predominates."[140]

This conclusion, which states essentially the view of Koenig, is no doubt *altogether too hasty*. While many good authorities agree with it in general, many others, on good grounds, consider that the experiment is by no means conclusive. "The question must still be regarded as an open one," is the opinion

[137] "But the possibility remains," says Lord Rayleigh, "that overtones, not audible except through their beats, may have arisen within the ear by transformation. This is the view favored by Bosanquet who has also made independent observations with results less difficult of accommodation to Helmholtz's view." *Op. cit.*, Vol. II., p. 469.

[138] He first used disks, later the hoops described above.

[139] Koenig, 'Ueber die Ursprung der Stösse und Stösstöne bei harmonischen Intervallen,' *Wied. Annal.*, XII., 1881, pp. 335–349.

[140] Zahm, *Sound and Music*, p. 338.

of Lord Rayleigh.[141] Concerning the views, held by the followers of Koenig, that the ear is capable of recognizing as a tone any periodicity within certain limits of frequency, Rayleigh suggests that a periodicity with frequency 128 is also periodicity with frequency 64. Is the latter tone to be heard as well as the former? Pipping, in 1895, urged the same point, that every periodicity of n is also a periodicity of $n/2$, $n/3$, $n/4$, etc.[142] "So far as theory is concerned, such questions are satisfactorily answered by Ohm's law," continues Lord Rayleigh. "Experiments may compel us to abandon this law, but it is well to remember that there is nothing to take its place."[143]

Koenig's wave siren experiments showed that at a certain rate of frequency both the beats and the tones, which he supposed to be generated by them, are heard at the same time. Koenig thinks that this is not necessarily contradictory to his view.

It must be kept in mind that Koenig still held to the *resonance hypothesis,* as did Young and others before Helmholtz, but did not accept Ohm's law. On this view, then, as explained by Zahm above, waves slightly diverging from pendularity are still sensed as tone. They are thus experienced, however, with much more difficulty than are pendular vibrations, while the beats are more easily perceived as distinct. In his later French publication[144] Koenig says on this matter: "At all events the simultaneous perception of separate beats and the sound which results from their succession is no more in contradiction with the new hypothesis than with the old [*i. e.,* Ohm's law as accepted by Helmholtz], for we can very well suppose that, besides the general excitement of the basilar membrane due to each separate beat, the particular parts of this membrane, whose proper tones correspond to the period

[141] Rayleigh, *ibid.,* p. 469.

[142] Pipping, Zur Lehre v. d. Vocalklängen, *Zeitschr f. Biologie,* XXXI.; N. F., XIII.

[143] Cf. Max Meyer's attempt at explaining another manner of analysis, page 92 f. of this paper.

[144] *Quelques Expériences d'Acoustique,* p. 137, trans. by Ellis in *Sensations of Tone,* p. 535.

of the impulses, are more strongly shaken, and excite lasting vibrations giving the perception of sound."

It will be recalled that Helmholtz had to account for all beats of wide intervals by the presence of upper partials or of difference tones. Now Koenig supposes that he has additional evidence against this view. From his approximately pure tones obtained from the resonated clangs, of large tuning-forks, he had heard beats that would require the presence of upper partials, which it would be entirely unreasonable to suppose were present.[145] Now, by means of the perfectly pure tones (as he supposed)[146] of the wave siren, he heard not only beats but also beat-tones, which according to Helmholtz's view would require the presence of upper partials. *E. g.*, 8:11 gave two tones 3 and 5 ($11 - 8 = 3$ and $2 \times 8 - 11 = 5$). 8:23 gave tone 1 ($3 \times 8 - 23 = 1$). But none of these tones are inexplicable from the formulæ of Helmholtz and Bosanquet.[147] And these formulæ resulting from the development of lower powers only do not exhaust the possibilities. In these cases, too, the resultant tones *do not depend upon upper partials, whether subjective or objective.* Even if no tone had before been heard corresponding to $3q - p$, *e. g.*, it is not an argument against the possibility that such tones exist, that Koenig now hears such a tone! Such tones (if indeed Koenig was not actually mistaken in the pitch, as others had been in regard to combination tones before him) are extremely rare whatever theory is adopted; and so far as the mere *existence* of these tones is concerned, they present no difficulty at all to Helmholtz's theory. Lastly we come to an important point in connection with this objection to Helmholtz's position. *How is Koenig to explain the perception of these tones?* It is well known that by means of his manometric flames and his complex curves, he finds periodicities corresponding to them; now, if he endeavors to make a mathematical or theoretical statement of these periodicities will he not actually obtain formulæ like those of Helmholtz and Bosanquet?

[145] Yet when Koenig comes to account for summation tones he posits the presence of partials of still much higher order! Cf. below pp. 53 f.

[146] But see note p. 48, *supra,* quoted from Rayleigh.

[147] Cf. p. 18 *supra.*

We have now to consider more especially the relation of the so-called beat-tones to combination tones. Koenig says that the beats (*i. e.*, the 'lower beats') are heard directly only to about the interval 4:7,[148] and, of course, beat-tones are heard only for smaller intervals. (The fact here is contradicted by recent experiments, as we shall see.) The interval $c^3:b^3$ (8:15), *e. g.*, gives only the tone 1, he says, and no trace of the tone 7 which Helmholtz's theory requires. So also with intervals beyond the octave; *e. g.*, the interval $c^3:d^4$ (4:9) gives only the tone 1 and never the tone 5; and $c^3:f^4$ (3:8) gives only 1 and 2. In short, the first difference tone of wide intervals, which Helmholtz's theory requires, is never heard by Koenig. None of Koenig's beat-tones fall between the primaries. Again there are no beat-tones corresponding to Helmholtz's *summation* tones. If then such difference tones and summation tones as Helmholtz believes in, exist at all, they are, for Koenig, distinct from the beat-tones. Only in certain cases do they coincide and in such cases their intensity is due largely to the presence of the beat-note of the same pitch.[149] "It follows," concludes Koenig, "that in all cases the differential tones must be infinitely more feeble than the tones resulting from the beats. But I was able to establish the actual existence of these differential tones with certainty by forming the above intervals with deeper notes, which, lasting longer, allowed me by means of auxiliary forks to get a definite number of beats with the differential tones in question."[150] In this paper, then, he does not deny the existence of combination tones, but holds only that they are very weak.

The summation tones, which Helmholtz heard easily from the siren and harmonium, Koenig explains as beat-tones of upper partials. These summation tones, while they may actually exist would be too weak to be heard. Now it is known, he points out, that the tones of the siren and of the harmonium

[148] *Quelques Expériences d'Acoustique,* note, p. 130.

[149] But cf. p. 105 below. Theoretically Helmholtz's theory includes all the tones heard by Koenig. It must be noted that Koenig did not always hear the tones required by his own theory, either.

[150] Koenig, 'Ueber den Zusammenklang Zweier Töne,' *Pogg. Annal.,* CLVII., 1876, p. 216. Quoted from Ellis' trans. in *Sensations of Tone.*

are not simple but are very rich in upper partials. The fifth, *e. g.*, with its two series of partials

$$\begin{array}{l} 2,\ 4,\ 6,\ \ 8,\ 10,\ \ldots \\ 3,\ 6,\ 9,\ 12,\ 15,\ \ldots, \end{array}$$

shows that the fifth partials will give a beat-tone ($15 - 10 = 5$) which is equal in pitch to the summation tone ($2 + 3 = 5$). The fourth (3:4) with the partials

$$\begin{array}{l} 3,\ 6,\ \ 9,\ 12,\ 15,\ 18,\ 21,\ \ldots \\ 4,\ 8,\ 12,\ 16,\ 20,\ 24,\ 28,\ \ldots, \end{array}$$

gives the tone 7 as a beat-tone of the seventh partials, and so on. In general the summation tones that are audible may be explained by the formula $n(a - b) = a + b$,[151] where a and b stand for the upper and lower primary tones respectively and n is some whole number.

In the French re-publication in 1882 the above observations still appear. Here, however, Koenig has enclosed in parenthesis the statement concerning his proof of the existence of combination tones by means of the beats of auxiliary forks, and on page 130 he has added the following long note. 'I have an important observation to make on all those remarks concerning differential and summation tones which are found in the first publication of the memoir in the *Annalen*, those which I have placed in parenthesis, but which I have reproduced here.

'New researches which I have made on beats of harmonical intervals since the publication of that memoir have demonstrated that even in forming very wide mistuned intervals, primary tones beat distinctly with a feeble auxiliary (excessive) tone; but, the auxiliary tones used to discover the existence of differential and summation tones, too feeble to be heard, were all harmonics of the tones of inferior beats, *i. e.*, of the lower beat-notes of the primary tones; and some of them, further, were harmonics of one of the primaries. They ought then, necessarily, to produce beats with these tones of which they were harmonics. Consequently from that time the beats

[151] Koenig does not seem to have used the formula $na - mb = a + b$; as a result he often has to posit the presence of very high upper partials to explain summation tones.

observed by me ceased to prove, in my judgment, the existence of corresponding tones interfered with (alteré) by the auxiliary forks, as I had previously held that they did.[152]

'A single exception should be made in case of the auxiliary of 440 v. s. which with the primary notes Ut_3 (8) and Si_3 (15) demonstrated by the presence of beats the existence of a feeble tone 7, which, however, was nothing but the tone of inferior beats (*i. e.,* the lower beat-note) of 8 and 15. Indeed the tables given above[153] show that the beats as well as the lower beat-notes of the first period can often be heard directly up to the neighborhood of 4:7, and it is conceivable that by the aid of an auxiliary tone, one should perceive them a degree further (14:15).

'After these considerations there remain only the two tones Mi_4 (5), produced by the interval Ut_3:Sol_3 (2:3), and $Ré_4$, from the interval Ut_3:Mi_3 (4:5), of which the existence may be regarded as really proved; because they have been observed directly by the ear. These tones, not having shown any action on resonators, as I have already indicated, cannot have the origin which Helmholtz attributes to differential and summation tones;[154] but, being very feeble, while the primary tones in the intervals which form them have a great intensity, they may be explained by the action of the feeble harmonics, produced in the ear by the primary tones; for according to Helmholtz, every very strong tone, even though simple, should produce harmonics in the organ of hearing, principally on account of the asymmetrical structure of the tympanic membrane, and partly on account of the loose articulation of the hammer with the anvil.[155]

'After what precedes, I know at present of no experiment by which one can prove with any certainty the existence of differential and of summation tones.'

In consequence of this 'discovery' he has revised note III.,

[152] It will be recalled that Koenig's experiments on beats lead him to the conclusion that one tone beats with its twelfth, *e. g.,* even though there are no upper partials present.

[153] *I. e.,* his tables on beats in *Quelques Expériences d'Acoustique.*

[154] This does not at all follow from Helmholtz's view.

[155] See my comment on this peculiar attitude of Koenig, page 56, below.

6,[156] of the 'conclusions' at the end of the chapter to read: 'The existence of differential and summation tones cannot at present be demonstrated with certainty by any experiment.' The original of this note (given in a footnote) reads: 'The differential and summation tones which are produced by the concurrence of two very strong tones, because the vibrations of the latter cease to be infinitely small, constitute a phenomenon which is independent of beats and of beat-tones.'

W. Preyer very early[157] raised the objection to Koenig's explanation of summation tones, that in some cases it requires the presence of a great number of overtones which apparently are not present. He cites the case of the interval 496:528 v. d. (31:33) of which he has heard the tone 1,024 (64). This would require the presence of the 32d partials, of 15,872 and 16,896 d. vib.[158] 'With tuning forks of 192 and 256 d. v. [3:4],' says Preyer, 'the summation tone 448 is heard clearly, even though both forks have been damped [to eliminate upper partials].' Now in this case it is very improbable that the seventh partial tones, after damping of the forks, would be loud enough to produce an audible beat-tone. In consequence of difficulties of this sort in the way of Koenig's view, Preyer refuses to accept it as a probable explanation. He is, however, of the opinion that these tones, though corresponding in pitch to summation tones, are too loud to be such. On the suggestion of G. Appunn, therefore, he shows that they may be explained as difference tones of the *second order; i. e.*, difference tones arising from the action of a difference tone on an upper partial. Thus instead of by means of the thirty-second partials of the tones of the intervals (31:33) referred to above, the summation tone can be accounted for as follows:

$$2 \times 528 - (528 - 496) = 1{,}056 - 32 = 1{,}024,$$

or

$$3 \times 528 - (2 \times 528 - 496) = 1{,}584 - 560 = 1{,}024.$$

[156] *Quelques Expériences d'Acoustique*, p. 147.

[157] *Akustische Untersuchungen*, 1879.

[158] Koenig thinks Preyer was deceived in the pitch in this case, that he heard really the octave of one of the primaries. Koenig, *Quelques Expériences d'Acoustique*, p. 127, note.

'Every term of these acoustical equations is easily proved [to exist].' Preyer's general formula then is expressed thus:

$$nb - [(n-1)b - a] = a + b.$$

Only the first overtone is necessary; *n*, then, $= 2$ and the formula becomes simply

$$2b - (b - a) = a + b.$$ [159]

When, however, b becomes greater than $2a$ the summation tone must be explained as a difference tone of the '*third*' order,

$$[nb - (n-1)a] - [(n-1)b - na] = a + b$$

or, when we consider only the first overtone,

$$(2b - a) - (b - 2a) = a + b.$$

'Since thus far neither I myself nor any one else has ever heard the summation tones when the first overtone was not at the same time very plainly audible, it is natural,' he says, 'to conceive of the summation tones as difference tones according to the above formulæ.'[160]

Now Koenig in turn, and on good grounds, objects to Preyer's explanation. To quote: "M. Preyer cites in favor of his views that on sounding together free reeds of 496 and 528 d. vib. $= 31:33$, he heard the sound 1024 d. vib. $= 64$, and he thinks that we cannot assume that the reeds had the thirty-second partials, 16,896 and 15,872 d. vib. If the sound really observed was 64, and not the octave of 31 or 33, we might be really astonished that the thirty-second partials were sufficiently strong in these tones to produce it; but the explanation proposed by M. Preyer is absolutely inadmissible, for 496 and 528 d. vib., even when they have considerable force, give 32 beats, which do not as yet allow the deep tone C_1 to be heard, so that at any rate such tone must be extremely weak. Now the octave of 528 (or 1,056) is the 33d harmonic of this excessively weak sound. But two primary sounds of 32 and 1,026 d. vib., even when extremely powerful, never pro-

[159] Röber (1856) first suggested this explanation, then independently G. Appunn, R. Fabri, and others. Cf. Stumpf, *Tonpsychologie*, II., 1890, p. 254.

[160] *Wied. Annal.*, XXXVIII., 1889, p. 135. I have not seen Preyer's earlier work.

duce a sound of 1,024 d. vib. The second manner in which M. Preyer thinks the sound might have been produced is equally opposed to all that has been directly observed when two primary tones sound together. Thus he makes the octave of 528 (*i. e.*, 1,056) produce with 496 d. vib. a differential tone of 560 d. vib., and then makes this tone 560 produce with the twelfth of 528 (*i. e.*, 1,584) a new differential tone of 1,024. But these two sounds of 496 and 1,056 ($= 2 \times 496 + 64$) give the beat note 64 and not 560; and if the sound 560 really existed, it would give with 1,584 ($= 2 \times 560 + 464 = 3 \times 560 - 96$) the beat-note 96, and also more faintly 464, but not 1,024."[161]

It is interesting to note that Koenig, when he fails to find objective upper partials for this explanation of summation tones, falls back for support[162] upon the subjective upper partials determined theoretically by Helmholtz and Bosanquet. The summation tones are, however, *co-equal* with the upper partials and not *dependent* upon them. Koenig, then, is in the peculiar position of accepting some of the values of x obtained from Helmholtz's equation and of rejecting others that stand co-equal with them! He would be far more consistent to go back to his earlier view and grant the existence of *weak* combination tones, if nothing more.

SECTION 2. The Objectivity of Combination Tones.

Helmholtz, it will be remembered, had distinguished between combination tones that are *objective* and those that are *subjective*.[1] He had found that combination tones from primary tones of instruments having a common windchest were reinforced by resonators and that they were able to set in sympathetic vibration suitable membranes attuned to them. But even these he had found to be mostly subjective. Koenig in his first publication on the secondary phenomena of hearing,

[161] Translated by Ellis, *Sensations of Tone*, p. 536, from a footnote in *Quelques Expériences d'Acoustique*, pp. 127–8.

[162] Cf. *supra*, p. 53.

[1] Koenig, for one, had not kept this distinction clearly in mind.

when he still believed in the existence of combination tones, said that neither the combination tones nor the beat-notes described by him were reinforced by resonators.[2]

We have already quoted from Bosanquet, how by the use of his improved resonator, he shut out all tones but that to which the resonator was attuned, and that in so doing all beats and difference tones disappeared, thus proving them to be subjective.[3] Preyer, too, worked on this problem as to whether combination tones are objective.[4] He constructed for the purpose seven tuning forks of extraordinary delicacy: f 170⅔, c^1 256, f^1 341⅓, a^1 426⅔, c^2 512, f^2 682⅔, and g^2 768 d. vib. These forks form the ratios 2:3:4:5:6:8:9. They 'were so ready to vibrate on the slightest excitement that they could be experimented on at night only.' The three lowest forks had the following partials:

Fork f had the 1st upper partial f^1 strong, also the 2d c^2, and the 3d f^2 weak.

Fork c^1 had the 1st and 2d c^2 and g^2 strong.

Fork f^1 had the 1st f^2 strong.

The forks c^2 and f gave f^1 or $6 - 2 = 4$; f^2 and f gave c^2, or $8 - 2 = 6$; g^2 and c^1 gave c^2, or $9 - 3 = 6$; 'and that these tones were objective enough was shown by their making the forks f^1 and c^2 vibrate sympathetically. But we see that f^1 and c^2 are partials of f and c^1, which existed already strongly on those forks, and if the forks f and c^1 were sounded separately, they also made the forks f^1, c^2 vibrate sympathetically. Hence the results did not prove the objective existence of the differential duplicates. On the other hand the forks giving the audible differential tones—

$f^2 - c^2 = f$ or $8 - 6 = 2$	$a^1 - c^1 = f$ or $5 - 3 = 2$
$g^2 - c^2 = c^1$ or $9 - 6 = 3$	$a^1 - f = c^1$ or $5 - 2 = 3$
$g^2 - a^1 = f^1$ or $9 - 5 = 4$	$g^2 - f^2 = a^1$ or $9 - 4 = 5$

$$c^2 - f^1 = f \text{ or } 6 - 4 = 2$$
$$f^2 - a^1 = c^1 \text{ or } 8 - 5 = 3$$
$$f^2 - c^1 = a^1 \text{ or } 8 - 3 = 5$$

[2] *Pogg. Annal.*, CLVII., 1876, p. 221.

[3] *Supra*, p. 26.

[4] M. Preyer, *Akustische Untersuchungen*, 1879, II.

utterly failed to produce the slightest effect on the forks having the same pitch.'[5]

By a similar test Preyer satisfied himself that none of these forks gave objective summation tones. He did not, in fact, even find *subjective* summation tones, *i. e.*, he did not hear *any* summation tones. "Perhaps," says Preyer, "they might be made audible after properly arming the forks by means of resonance boxes *while* sounding. But the observation would not be easy."[6]

While Preyer's general conclusion is that combination tones are subjective, he acknowledges in a later article, from experiments which he there mentions, that appropriate conditions may be set up externally to the ear and made to generate combination tones.[7] Membranes, he says, may supply such conditions.

One of the experiments to which Preyer referred was that of O. Lummer,[8] in which, in Helmholtz's presence, Lummer made a microphone resonator respond perceptibly to the summation tone of primaries generated by the harmonium.

In the same year as Lummer's experiment, M. Wien obtained only negative results. He found that a very delicate resonator was never sensibly affected either by the loud difference tone of the primaries generated by two Quincke tubes (Lippenpfeifen) or by a tube and a telephone.[9]

L. Hermann, in 1891, reports an experiment bearing on the same problem of objectivity of combination tones. He admits that instruments with common windchest for both primary tones give objective difference tones, and therefore holds that Lummer's experiment is not to the point since he used the harmonium. The question, as clearly stated by Hermann, is, whether primaries of independent sources generate objective combination tones. 'I inserted,' says Hermann, 'two tele-

[5] Note that some of these 'audible differential tones' are intermediate difference tones.

[6] From Ellis, in *Sensations of Tone*, pp. 531–2.

[7] Preyer, Ueber Combinationstöne, *Wied. Annal.*, XXXVIII., 1889, p. 133.

[8] 'Ueber eine empfindl. obj. Klanganalyse,' *Verhandl. Berl. phys. Gesellsch.*, 1886, p. 66.

[9] Max Wien, 'Ueber Messung der Tonstärke,' *Wied. Annal.*, XXXII., 1889, p. 853.

phones into the same circuit with the electro-magnet of a Koenig secondary electric tuning fork, which was attuned to the tone *F*. The telephones *A* and *B* were in a separate room. Their steel membranes were removed and two assistants held the Koenig tuning forks a^1 and c^2 as near as possible to the coil. Never, however, was it possible in this way to bring into sympathetic vibration the fork *F* which corresponded to the difference tone of a^1 and c^2; but if another *F* fork was brought near one of the telephones, the electric fork vibrated weakly. On the other hand, if instead of the electric fork *F*, a telephone receiver (Hörtelephone) was inserted into the circuit, one could hear in it the difference tone *F* most beautifully; and likewise if, instead of a^1 and c^2, other forks were used, their difference tones or beats, as the case may be, were also heard. But this last experiment did not, indeed, prove that the plate of the telephone receiver produced or strengthened the difference tone; it simply reproduced to the ear simultaneously the two primary tones, and it was these which subjectively produced the difference tone.'[10] His experiment, then, seemed to show that generators of independent sources do *not* produce objective combination tones.

In 1895 the physicists A. W. Rücker and E. Edser[11] took up the problem and performed an experiment which has become classic. These men were stimulated to the investigation partly, at least, by the statements of Ellis in notes, pp. 156 and 157, of Helmholtz's *Sensations of Tone*. Ellis says there that it is probable that the 'apparent reinforcement' of the resonators noticed by Helmholtz, in case of combination tones from primaries generated by the siren or harmonium, 'arose from imperfect blocking of both ears when using them.'[12] "These statements are unqualified," they say, "and no condition was made as to the way in which the combination tones were pro-

[10] L. Hermann, 'Zur Theorie der Combinationstöne,' *Archiv f. d. ges Physiol.*, LIX., 1891, p. 516.

[11] Rücker and Edser, 'Objective Reality of Combination Tones,' *Phil. Mag.*, Series XXXIX., 1895, pp. 341–57.

[12] Ellis held the view of Koenig (for a time) and Bosanquet, and the one which Preyer also held for a time, that *all* combination tones (not 'beat tones,' of course) are subjective.

duced." They decided to make a careful test in which the ear need not be employed directly at all.

They used a tuning fork as resonator, a Koenig tuning fork of 64 d. vib. Since this instrument is relatively difficult to excite by resonance, they used a very delicate method of detecting whether it was set in motion. For this purpose a mirror was attached to one of the prongs and a system was formed by which the Michelson interference bands were produced. "A movement of the prong amounting to half a wave length of light (say 1/80,000 of an inch) would alter the length of the path of one of the interfering rays by a wave length. A periodic vibration of this amplitude would cause the band to disappear." The bands were sometimes produced by a sodium light, and sometimes by an electric light. They were watched by an observer through a telescope. A movement of "one hundred thousandth of an inch could easily be detected."

They first experimented with combination tones of primaries generated by the siren, i. e., where the primaries had a *common* wind supply. The experiment seems to have been conducted with the greatest of care. By accurate control of the revolutions made by the siren disk they could produce combination tones of pitch equal to that of the resonating fork. Many experiments were performed with different frequencies of the primaries, but in every case where the primaries formed intervals not greater than the octave, the fork was 'powerfully affected' when the difference tone had a frequency equal to its own.

The experimenters tested also for the first difference tone of an interval greater than the octave (4:9). According to Koenig's rules there are no 'beat-tones' of pitch intermediate between the primaries. The result of the experiment was unmistakable. When the primary tones reached frequencies such that the difference, 5, corresponded to 64 vibrations the resonating fork was disturbed as usual. "The effect was rather feebler" than in the other experiments with intervals smaller than the octave, "but there was absolutely no doubt as to the objective reality of the difference tone. The bands

regularly disappeared when the required pitch was obtained, and reappeared when it was lost."[13]

They tested next for Koenig's *lower beat-tone* with intervals greater than the octave. They produced for this purpose the tones 256 and 576 ($576 - 2 \times 256 = 64$ the pitch of the resonator fork). The results were always negative. "We lay less stress on negative than on positive results," they say; "but we tried for a long time on two occasions to get evidence of the objective character of the note, but entirely failed."[14]

They next turned their attention to the summation tone, still using the polyphonic siren with common windchest. Although the primary tones had to be so low that their sum = 64 v. d., the results were always unmistakable. When the pitch of the summation tone equaled that of the resonating fork the bands invariably disappeared. The experiments "left in the minds of those who saw them no shadow of doubt as to the objective reality of a note corresponding in frequency to the summation-tone."[15]

Koenig, it will be remembered, explained summation tones as 'beat-tones' of upper partials. Usually the 'beat-tones' of the lower harmonics are equal in pitch to the primary tones or to some of their upper partials. This is true, *e. g.*, of the fifth (2:3)

2, 4, 6, 8, 10
3, 6, 9, 12, 15.

Here $6 - 4 = 2$, the lower primary tone; $9 - 6 = 3$, the upper primary; $12 - 8 = 4$, the first upper partial of the lower primary tone; and $15 - 10 = 5$, the first such beat-tone of upper partials that we could expect to hear by itself.

The case, though, is somewhat different with the fourth (3:4)

3, 6, 9, 12, 15, 18, 21
4, 8, 12, 16, 20, 24, 28.

Here the 'beat-tones,' if such there be, of the first and fourth

[13] *Ibid.*, p. 350.
[14] *Ibid.*, p. 530.
[15] *Ibid.*, p. 351.

upper partials should be more prominent, presumably, than the 'beat-tone' of the sixth upper partial, which corresponds to the summation tone. Rücker and Edser found the tone 7, *i. e.,* the summation tone, to exist objectively, and decided now to test also for the tone 5 (from 20 — 15). This ought, according to Koenig's explanation, to be stronger than 7. "When the speed corresponding to this difference-tone [*i. e.,* 5] was attained, there were occasional flickers of the bands, so that it is possible that it has an objective existence. But on the other hand, the effect was less than that produced by the summation tone [*i. e.,* 7]. The bands never disappeared for any considerable length of time, as they did when the forks responded to the summation tone, and the experiment left no doubt in our minds that the greater effect was produced by the summation tone."[16]

Another slightly different test was made, bearing on the same problem, as to whether summation tones are due to upper partials. Not all primary tones have ratios that make it possible to explain the summation tone as produced by partials of the same order, *i. e.,* the equation

$$a + b = n(a - b)$$

does not always apply. If the primary tones have the ratio 9:16, *e. g.*, then $a + b = 25$ and $a - b = 7$. "The 10th partial of the upper note beating with the 15th of the lower note ($160 - 135 = 25$) would indeed have the same frequency as the summation tone, but it appears to be absurd to suppose that so improbable a combination should produce appreciable results. . . . If we assume that any pair of partials can thus produce objective tones, the number of combination tones will be so great that the fork ought to have been disturbed frequently when the note of the siren was being raised to the required pitch. *As a matter of fact when the C of 64 vibrations was passed, so that all the partials were higher than the pitch of the resonating forks, no such disturbances were ever observed except when the difference- or summation-tone of the primaries was produced.* Putting, therefore, all such fantastic

[16] *Ibid.,* p. 352.

combinations aside, the experiment may be regarded, as a test whether the summation tone can be produced when it cannot be due to two partials of the same order."

Testing, then, for the summation tone of the primaries of the ratio 9:16, they found, by the disappearance of the bands, that the summation tone, as in the previous cases, did actually exist objectively. To make sure that it was not the third partial of the lower tone, however, they made also a test for the partial. This partial was found to shake the bands a little when the rate of rotation made it equal to the pitch of the resonating fork, "*but the bands did not disappear, whereas they were completely wiped out by the summation tone when the two notes were sounded.*" Both by this means and by the difference in pitch of the two tones (one 25 and the other 27) the experimenters convinced themselves "that the effect of the two sources of disturbance could be distinguished, and that the supposed summation tone was not due to the partial of the lower note."[17]

Also by means of a *mirror-resonator,* constructed by Professor Boys to respond to a vibration frequency of 576, the experimenters demonstrated the objective existence of summation tones from primaries generated by a double siren with a common wind chest. These tests were made with primaries having the ratios 4:5, 3:4, and 9:16. In every case the summation tone disturbed the mirror.

"We attach great importance," say Rücker and Edser, "to this corroboration of our results by an instrument of totally different construction from that first employed."[18]

Experiments were also performed in which the primary tones were generated by tuning forks, but the results were negative. "No effect whatever was produced [on the resonating fork], and there can be no doubt that if objective combination tones are produced in such cases they are much less intense than those generated by the siren." Tests were also made with reed tones and with tones of organ pipes. In the first case the results were uncertain; in the second they were negative.

[17] *Ibid.*, pp. 352–4 (italics mine).
[18] *Ibid.*, p. 354.

Several attempts were made to detect combination tones of higher orders, such as $2q + p$ and $2q - p$, but without success.[19] No statement is made as to how the primary tones were generated in these cases. They were presumably tones from the siren.

Rücker and Edser conclude, on good grounds, that Helmholtz was right in ascribing objective reality, in part, to the first combination tone ($p \pm q$) of primary tones produced by the siren. So far as the negative results are concerned, it can only be said that the *instrument used* was unable to show any objective reality of the tones sought.

Forsyth and Sowter,[20] by means of photography, registered the sympathetic vibrations of a small mirror to combination tones generated by the siren. The instrument was so delicate that they found it necessary to make their tests at night when traffic had subsided. The mirror was attuned to 64 vibrations per second. The primary tones were produced by a Helmholtz polyphonic siren. When a 64-fork was sounded, the experimenters obtained (by instantaneous photography) a series of sinus lines, which were used for comparison with results obtained both from difference tones and from summation tones when the frequencies equaled that of the natural period of the mirror. The photographs show that the summation tone was somewhat weaker than the difference tone. Only a few intervals were used, but there remained no doubt as to the results obtained.

In 1899 K. L. Schaefer records[21] results of experiments which he performed on the harmonium and the Appunn triad apparatus (Dreiklangapparat). He was able to reinforce with resonators not only the first combination tones ($p \pm q$) but also the so-called difference tone of the second order ($2q - p$). In relation to the intensity of the tone, the rein-

[19] *Ibid.*, pp. 355–6.

[20] R. W. Forsyth and R. J. Sowter, On Photographic Evidence of the Objective Reality of Combination Tones. *Proceedings of the Royal Society of London*, LXIII., 1898, pp. 396–399.

[21] K. L. Schaefer, 'Eine neue Erklärung der sub. Combinationstöne auf Grund d. Helmholtz'chen Resonanzhypothesis,' Pfluger's *Archiv*, LXXVIII., 1899, 508–526.

forcement was not great but unmistakable. Schaefer found, too, that no reinforcement was possible when the primaries were generated from separate wind chests. He further agreed with Helmholtz's results in finding that the degree of strengthening of the combination tones by resonators is always small in comparison with the intensity with which the tones are normally heard without resonators. The greater part of their intensity, he concludes, is, therefore, not of objective origin.[22]

From the above experiments bearing on the question of the objectivity of combination tones it may safely be inferred that if combination tones from primaries generated independently exist objectively at all, they are extremely weak. Lord Rayleigh assures us, however, that owing to the want of symmetry due to condensation and rarefaction in the air "the formation to some degree of octaves and combination tones is a mathematical necessity."[23]

It is at once evident that the question of the origin of the secondary phenomena of hearing is closely connected with—is indeed only another phase of—that which we have just been discussing. If the combination tones, *e. g.*, could all be shown to exist objectively the question as to their cause might well be left entirely for physicists to answer. It is an appreciation of this fact that has caused several acousticians in recent years to investigate rather carefully the question of objectivity with reference to various so-called resultant tones. We shall return now to the subject of 'interruption tones' and of variation tones and see what effect the question of objectivity has had on the development of their theoretical treatment.

[22] K. L. Schaefer, 'Weiter Bemerkung zu meiner neuen Erklärung, etc.,' Pflüger's *Archiv*, LXXXIII., 1900, 73–78, cf. pp. 75–6.

[23] *Theory of Sound*, II., 1896, p. 459.

SECTION 3. Later Experiments on Interruption Tones.

We saw that the earlier studies of the periodically variable tones were made by physicists, and that they had come to a rather general conclusion that if a tone n is interrupted periodically m times in a certain period of time, other tones, corresponding to $n + m$ and $n - m$ should arise as actual pendular vibrations in the air; and that in some cases these tones had actually been located as to their pitch by the aid of resonators, though the question of objectivity had not been explicitly considered. Several investigators had also noticed a tone corresponding to m, though the results of the mathematical determinations did not show such a vibration. The tone m, moreover, had apparently never been resonated objectively.

In 1887 H. Dennert[1] took up the problem where Koenig had left it. Dennert's own description of method and results follows: 'On a disk with three circles which were each divided into 96 equal parts,' he so constructed holes of equal diameter 'that on the first circle 4 parts that were perforated regularly alternated with 4 unperforated ones; on the second circle 3 perforated parts, with 3 unperforated ones; on the third 2 perforated, with 2 unperforated parts. Therefore on the first circle were twelve groups of 4 holes with 4 blank spaces between each group, in the second 16 groups of 3 holes with 3 blank spaces between each group, and on the third 24 groups of 2 holes with 2 blank spaces between each group. Now if the circles were blown upon [through a tube] during the rotation of the disk, one heard, with slow rotation, beats which, with more rapid rotation, merged into tones. The beats of the third circle first went into a tone, then those of the second, and lastly those of the first. In every phase of the investigation the tones stood in the relation 1:4/3:2, so that when the lowest of the tones from the beats was equal to c, one heard, besides the tone c^3, which corresponded to the 96 holes, also the three tones in the series c, f, c^1.'[2]

[1] H. Dennert, 'Akustische-physiol. Untersuchungen,' *Archiv für Ohrenheilkunde*, XXIV., 1887, pp. 171 ff.

[2] *Ibid.*, p. 181.

For Dennert these last three tones, c, f, c^1, had no corresponding pendular vibrations in the air, but, as for Koenig, were supposed to be generated from rapid beats due to the periodic interruptions of the tone c^3.

Hermann, also a supporter of the 'beat-tone' view, next took up some rather extended experiments on the 'interruption' tone and on allied problems. First a constant tone f^2 was conducted through a tube to the observer's ear and a revolving disk with a circle of 18 holes of 16.5 mm. diameter was arranged between two sections of the tube so that it periodically interrupted the tone. When the rotation was rapid enough the beats of intermittence merged into a continuous low tone, which, on account of the noise of the rotating disk, was not very clear. When the rotation was sufficiently increased the high tone f^2 became entirely inaudible.

In a second experiment a tone transmitted by telephone was interrupted by means of a water motor. Again when the interruption was sufficiently rapid a low dull tone was heard corresponding in pitch to the number of interruptions. With greater frequency of interruption this low tone also completely drowned out the high tone. In both of these cases, however, the low tones disappeared when the high tone was stopped,[3] showing their dependence upon it.

In another experiment Hermann employed four of the Savart toothed wheels (Zahnräder) having 80, 60, 50 and 40 teeth respectively. The wheel with eighty teeth had ten blank spaces which divided the teeth into eight groups with equal distances between each two successive teeth. With each revolution, therefore, there would be ten interruptions of the tone produced, when the edge of a piece of paper was brought into contact with the teeth. When the wheel was rotated ten times per second, *e. g.*, there was heard, besides the higher tone of 800 vibrations, also a low tone corresponding to 100. In this case also the high tone disappeared on very rapid rotation, while the lower, or 'interruption tone,' only was heard. Now when the disk with 60 teeth was likewise arranged for ten interruptions per revolution, it was found, on making an equal num-

[3] Hermann, *op. cit.*, pp. 385–86.

ber of revolutions with the first, to produce a low tone of the same pitch as that of the first disk. The two higher tones were, of course, different, bearing the ratio 3:4. By the use of the remaining disks several experiments were performed with similar results. After a rapid rotation one notices, as the disk gradually loses speed, that the upper tone comes out more clearly as the lower one falls in pitch. Finally the lower one ceases and only beats of the interruptions are heard. Very rapid rotation always drowns out the high tone, as in the previous experiments. It is clear from this change of the lower tone into beats, argues Hermann, that the ear perceives rapid beats as a continuous tone.[4]

W. Voigt,[5] at about the same time that these experiments were in progress, endeavored to construct a theory on the basis of mathematical determinations, which should take into account two points in which he thought the theory of Helmholtz was insufficient. (1) Koenig and others had shown to Voigt's satisfaction that every periodic fluctuation in the air was sensed by the ear as a tone.[6] This, of course, the Helmholtzian theory denied. (2) *Difference tones,* as Voigt himself found, *do not always have the intensity that Helmholtz's theory demands.* On the assumption, then, that any periodicity may be sensed as a tone, and rejecting the disturbance of vibration by superposition, Voigt obtains results that admit both of Helmholtz's combination tones and of Koenig's 'beat-tones.' Really, however, as Krueger suggests,[7] Voigt gives up the Ohm-Helmholtz theory of analysis without explicitly recognizing it. Voigt finds from theoretical considerations that the summation tone should not exist for intervals of the octave, the fifth, the fourth and the third, and that its existence in the case of the major sixth, and the twelfth is very questionable.

[4] *Ibid.*, pp. 386–7.

[5] 'Ueber die Zusammenklang zweier einfacher Töne,' *Wied. Annal.*, XL., 1890, pp. 652–60.

[6] This statement must be understood as applying to the rates of vibration within audible limits, *e. g.*, 16 to 50,000 per second.

[7] *Phil. Studien,* XVII., 1901, p. 265.

SECTION 4. FURTHER CRITICISMS AND MODIFICATIONS OF HELMHOLTZ'S VIEW.

Hermann now[1] decides that Helmholtz's theory, 'beautiful as it is' is inadequate to explain the empirical facts that have been gathered. Koenig, Dennert, and himself have found that the beats of periodic interruption produce sensations of continuous tone; Koenig found that the difference tone corresponding to $(p - q)$ is heard only for intervals not much wider than half (?) an octave. Voigt has pointed out that the intensity relations of combination tones do not agree with the demands of the Helmholtzian theory; and Hermann himself "had never heard summation tones and had never found any one who could hear them" even under the most favorable circumstances.[2] In addition to Voigt's observation on intensity Hermann has himself observed that the forks $c^2:e^3$ on resonance boxes give the difference tone F on very gentle sounding. *'This simple fact in itself,'* he says, *'is sufficient to disprove the Helmholtzian theory of combination tones.'*[3]

But Hermann has still other objections to urge. Difference tones, he found, can easily be heard even when the ears are stuffed with cotton or filled with a wax compound,[4] so that the drum can not function as Helmholtz's theory demands that it should for the generation of combination tones, by transformation of the primaries. Furthermore, Hermann found that on having the tones of two tuning forks conducted one to each ear through pipes, he still heard both beats and combination tones.[5] In this case he supposed that the tones, through the mediation of the bones of the head, both act together in each ear, but that difference-tone origin in the drum is impossible.

Aside from these difficulties, Hermann also suggests that Helmholtz's resonance theory is improbable from the fact that it requires fibers hardly 0.5 mm. long to respond sympathetically

[1] 'Zur Theorie der Combinationstöne,' *Pflügers Archiv*, XLIX., 1891, pp. 499–518.

[2] *Ibid.*, p. 500.

[3] *Ibid.*, p. 512.

[4] *Ibid.*, pp. 512–3.

[5] See the interesting experiment by Cross and Goodwin, p. 75 ff., below.

to tones of less than forty vibrations per second. Such a thing he believes to be contradictory to what we know of sympathetic vibration.[6]

Now, in the face of this evidence against the view of Helmholtz, Hermann decides to abandon it. 'Nothing remains then,' he says, 'but to return to the old natural deduction of difference tones from beats, *i. e., to ascribe to the ear the power of responding with the sensation of tone to every kind of periodicity within certain frequency limits*.'[7] In this new creed he hoped to find peace of mind with the difficulties enumerated above.

But while we are compelled to put away the resonance hypothesis, the principle of the specific energies of nerves remains unaffected, he continues. No experiments have yet made the application of this principle to tone analysis improbable. We are forced, then, in the case of hearing, as we are in other senses, to abandon the hope of explaining *how* a certain tone exclusively or preferably stimulates a certain nerve.

Now with this view that the ear perceives as tone not only pendular vibrations, but also *any* periodic vibration, within certain frequency limits, Hermann took up a study of other possible tones.[8]

If we construct the resultant of two curves of equal amplitudes representing frequencies of m and n we obtain a curve that approaches sinuosity, with the exception that its amplitude fluctuates periodically from zero to the sum of the amplitudes of the curves m and n. The period of this resultant curve corresponds to the arithmetical mean $(m + n)/2$ of the primaries and is, therefore, by Hermann, called the 'Mittelton.'[9] Koenig referred to it as the 'son moyen.'[10] If now m and n are the vibration numbers of the primary tones respectively in 2π seconds, and if these tones are conceived as cosinus curves their formula, beginning with the opposite phase, is

$$a \cos mt - a \cos nt,$$

[6] Helmholtz anticipated this objection himself; see above, p. 13.

[7] *Ibid.*, pp. 513–4.

[8] Hermann, 'Beiträge zur der Lehre von der Klangwahrnehmung,' *Pflüger's Archiv*, LVI., 1894, pp. 467–99.

[9] *Ibid.*, p. 486.

[10] *Quelques Expériences d'Acoustique*, p. 143.

where m is greater than n; or by simple trigonometric conversion,

$$a(\cos mt - \cos nt) = -2a \sin \frac{m+n}{2} t \sin \frac{m-n}{2} t$$

In this case $(m+n)/2$ is the vibration number of the 'middle tone' and sin $(m-n)/2$ represents the intensity of the fluctuation of the tone, and evidently will change sign periodically. As Lord Rayleigh assures us,[11] *this change of sign* is equivalent to a *change of phase*. The 'middle tone,' then will periodically change its phase, *i. e.*, in every 2 seconds. It is evident that between each two phase reversals of this 'middle tone' only a relatively small number of vibrations can take place. The general expression of this number is obtained by dividing $\frac{1}{2}(m+n)$ by $(m-n)$ and is, therefore,

$$\frac{m+n}{2(m-n)}$$

as Hermann shows.

It is evident that this 'middle tone' with changing phase will not act on a resonator of high degree of resonance where the cumulative effect of a great number of impulses is important. In such a case, *e. g.*, as the resonance of a tuning fork, the second series of impulses with a reversal of phase would tend to neutralize the effect of the first. The effect, if any there be, of the 'middle tone,' for this reason, would be greatest in cases where the resonators would soon be damped, *i. e.*, where resonance is of a low degree. It will be easily understood now why the 'middle tone' is not susceptible of reinforcement by physical resonators. Still Hermann thought that he had occasionally heard this tone in cases of interference of primaries forming simple intervals.

By different arrangements of teeth in the toothed siren Hermann found (1) that periodic vibration may be perceived as a tone even though there is a regularly recurring change of phase which does not occur with too great frequency. For example, a periodicity changing its phase with every 16th vibration is sensed as a tone. The limits of tonal perception in

[11] *Supra*, p. 38; cf. also Hermann's figure, *Pflüger's Archiv*, LVI., p. 485.

such cases were found to be at about four vibrations between each change of phase. He found (2) that in cases of periodic change of phase, as well as in periodic intermittence, a tone may arise whose vibration number equals the number of phase reversals. Accordingly *he explained difference tones as resulting from this periodic fluctuation of phase and amplitude of the 'middle tone.'* The period of the fluctuation is expressed by the difference $m - n$, and, moreover, the number of the vibrations of the 'middle tone' within each phase becomes so small for wide intervals that it renders the existence of the difference tone in such cases impossible. He was thus able, on the basis of his precarious assumptions, to explain another phenomenon which he held as contradictory to the Helmholtzian theory.[12]

Now how shall we explain the fact that a periodicity may be sensed as a tone even though the change of phase occurs as often as once in every four vibrations [see (1) above]? If stimulation of the nerve endings is effected entirely by forced vibrations we should suppose that a change would not affect the tonal perception at all. On the other hand, if stimulation is effected by the presence in the cochlea of perfect resonators the effect of phase change ought to be *much greater than it is.* Hermann, though he had rejected Helmholtz's theory of difference tones altogether, decides to posit the presence of certain imperfect resonators in the ear. Since change of phase is opposed to resonance it is natural that the 'middle tone' should only seldom succeed in getting any sympathetic response in the cochlear resonators. Thus he accounts for the fact that this tone is seldom heard.

Now, however, he is confronted with the same difficulty that caused Helmholtz to reject the view that rapid beats merge into continuous tone. This view is opposed to specific energy of nerves and resonance, in that each nerve is affected only by its own resonator. To get around this difficulty Hermann assumes that each resonator operates not directly on the nerve, but upon the nerve cell and thus indirectly upon the nerve; and *that each of these cells is functionally connected with every*

[12] But how will *he* explain the fact that the intensity of the second difference tone sometimes exceeds that of the first?

resonator. It matters not, then, *where* in the series the vibration sympathetic to the given tone is, for every resonator communicates with the only nerve that can mediate the corresponding tone.

In 1896 Max Meyer reports[13] experiments on the toothed and also on the perforated siren that confirm the results of the similar experiments made by Hermann. But Meyer points out the fact that these experiments do not necessarily support the 'middle tone' theory of Hermann. The phase-changing vibrations produced on the siren are all of approximately equal amplitudes, whereas those of the 'middle tone' vary between 0 and a certain maximum value. Meyer found that on faster rotation of the siren the phase-changing tone gradually gave place to the variation tones which finally survived alone, while the former disappeared altogether. Meyer suggests that what Hermann heard and interpreted as the 'middle tone' may have been upper partials of the primaries which in some cases correspond to harmonics of the supposed 'middle tone.' *E. g.*, the 'middle tone' of $c^1 : g^1$ is e^1, which might have been suggested by the upper partial e^3.[14]

It is important to note that Hermann did not seem absolutely certain himself that he heard the tone in question. Any one who knows the force of imagination in trying to hear a weak tone of a clang, especially when the pitch of the expected partial is known, will feel inclined to doubt under the circumstances that Hermann actually heard the 'middle tone.' Krueger[15] says that the existence of such a tone becomes more improbable as experimental conditions approach the actual conditions demanded by Hermann's theory.

Wundt in 1893[16] suggests, as a supplement to Helmholtz's theory, an hypothesis quite different from the theory of Hermann. With Koenig, Hermann, and other, he agrees that Helmholtz's view cannot explain certain secondary phenomena of hearing, such as have been pointed out by Hermann. Cross

[13] M. Meyer, 'Ueber Combinationstöne,' *Zeitschrift f. Psychol.*, IX., 1896, 177–229.

[14] *Ibid.*, 196 ff.

[15] *Phil. Studien*, XVII., 1901, p. 270.

[16] W. Wundt. *Phil. Studien*, VIII., pp. 641–52.

and Goodwin[17] heard beats from soft tones not far separated in pitch, even when one tone was separately conducted to each ear, precautions being taken against the conduction through the bones of the head. Such results, holds Wundt, are impossible if each nerve fiber, however stimulated, can mediate only its particular sensation. Beats of wide intervals, such as Koenig heard, are inexplicable on this theory. These difficulties can be overcome, says Wundt, by the simple assumption that the *vibrations of both tones can somehow directly stimulate all the fibers of the auditory nerve,* without going to or acting through the resonance apparatus in the ear. In case of direct stimulation of this kind, such intermissions of stimuli as are objectively not in a condition to produce a tone could set up together the conditions necessary for a tone sensation.

This supposition is opposed to that of Hermann in that (1) it retains the resonance hypothesis as formulated by Helmholtz and also his explanation of beats and combination tones. 'For any mechanically comprehensible interpretation of the resonance hypothesis, this direct stimulation of the nerve of hearing is not in opposition to the resonance hypothesis, but must stand to complete it; the hypothesis demands it.' (2) It does *not* hold to specific energies of nerves in any strict sense. On both of these points, it will be remembered, Hermann took the position just opposite to that of Wundt. If structures attuned to repond to certain objective vibrations can stimulate the nerve fiber by sympathetic vibration, argues Wundt, why cannot the objective vibration directly stimulate the nerve? R. Ewald showed that animals from which the whole hearing labyrinth has been extirpated, still react to sound stimuli even in cases where the tactile stimuli were supposedly excluded.[18] Even though he attributes the response to *general* nerve stimulation, that does not exclude the kind of direct stimulation here assumed. While other nerves, *e. g.*, those of touch, are not excitable by so low intensity of stimulation, we may well sup-

[17] *Proc. of the American Acad. of Arts and Science,* XXVII, June 10, 1891.

[18] R. Ewald, *Physiol. Untersuchungen über das Endorgan des Nervus Octavus,* Wiesbaden, 1892, p. 29. It has usually been supposed by critics, however, that Ewald's animals reacted to tactile stimulation.

pose that the auditory nerve through adaptation is more sensitive to such stimulation.

On the anatomical side Wundt finds further confirmation of his view. The auditory nerve shows a divergence from all the nerves in that just before its peripheral termination it does not, like other nerves, become imbedded in soft tissue, but unravels itself into fine fibers surrounded with bony walls. This complexity of structure and this type of protection are unmeaning from the view that tonal analysis takes place solely in the arches of Corti. But, says Wundt, useless structures are not found in organisms (!). It is well known, moreover, that tones may be mediated through the bones of the head.

If, then, this 'supplementary hypothesis' is correct, every sensation set up by the direct stimulation of the auditory nerve will conform to the objective form of the wave. Simple tones will be sensed as simple; complex, as complex. In cases of interference, beats and beat tones will arise. Thus, not rejecting Helmholtz's theory, we may admit with Koenig that beats when frequent enough may be sensed as tones.

Of this 'supplementary hypothesis' of Wundt we need say only a few words. It is a strange mixture of the doctrine of specific energy of nervous function in the cochlea, with an entirely different view of the *modus operandi* of the *same nerves* where they pass through the bony structure. The contradiction of the 'supplement' with the specific energy theory of Helmholtz is so great as entirely to do away with the view which it is to complete! The two suppositions cannot keep house together on any known principles of the physiology of nerves. Moreover, Wundt himself concedes, later, on the basis of experiments which we shall soon consider, that the so-called beat-tones and intermittence-tones 'have themselves been reduced with great probability to difference tones.'[19]

It is important to note, in connection with the criticisms just urged against Wundt's "supplement," that the results of the experiment of Cross and Goodwin,[20] *taken in their entirety,*

[19] Wundt, *Physiol.-Psychol.*, 5th ed., Vol. II., p. 137.

[20] Chas. R. Cross and H. M. Goodwin, 'Some Considerations Regarding Helmholtz's Theory of Consonance,' *Proc. of the Am. Acad. of Arts and Sciences*, XXVII., 1891, pp. 1–12.

contradict Wundt's position, although Wundt takes consolation from a certain *part* of the results. We shall, therefore, examine a little more carefully the results and the conditions of this experiment.

An "effectual means of making audible very small vibrations is to close the ear with a bit of beeswax and press the stem of the fork lightly against the wax. In this case the vibrations are transmitted to the membrana tympani by the small amount of air enclosed within the meatus, as is clear from the fact that the sound of the fork is heard on touching the wax long after it ceases to be audible on touching its stem to the pinna of the ear. Hence in this case there is no conduction to the middle ear or inner ear through the bones of the head [this cannot yet be asserted beyond question]. Now we found that the vibrations of a fork could be heard longer when touched to the wax in the ear than when held against the teeth. We therefore took two small tonometer forks making four beats per second, struck them very gently, and held their stems against the teeth; loud beats were heard in the ears. . . . The forks were then held in this position until the beats had entirely ceased to be audible, when they were removed, and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats." The beats were correctly counted by the subject.

"The experiment was varied slightly as follows: One ear only was closed with wax; the other was immersed in a large basin of water. The experiment was then repeated as above, with the difference that one fork, instead of being touched to the ear was touched to the marble basin, its vibrations being transmitted to the enclosed ear through the water. The same results were obtained as before." The experimenters "conclude that aerial vibrations acting upon the ear are not transmitted through the skull or bony parts of the head from one ear to the other."

Upon this part of the results of the experiment, then, Wundt

largely bases his theory. But the experimenters go on, and their results agree in general, as they point out, with those of similar experiments by Thompson in 1881. "The ears being closed with wax, a brass rod about five feet long was held lightly against the wax in each. When the stems of forks struck by two assistants were pressed against the farther end of the rods, very loud tones were heard in the ears, unaccompanied by any differential tone. If, however, one of the rods was removed from the ear and pressed tightly against the head, or, better, against the teeth, a loud differential tone was heard at once in the ear against which the rod was placed. If both rods were held against the teeth or head, the differential tone was heard in both ears." Difference tones were, therefore, readily produced whenever bone conduction was made possible *so that both tones might operate in the same inner ear.* These results were confirmed by several other different tests which need not be described.

The experimenters conclude that while Helmholtz's explanation of beats may be partly right, the production of beats is in part due to the condition resulting in the sensorium itself when two interfering tones are sensed. They offer no explanation of the origin of combination tones, though they find, contrary to their expectations—based upon Helmholtz's explanation of asymmetry in the tympanum—that such tones arise even when it is impossible for both generating tones to operate together on either tympanum.

These results, bearing on the hearing of difference tones, certainly contradict the very fundamentals of Wundt's "supplement" to Helmholtz's theory. On the other hand, they are in agreement with the modification of this theory suggested below, page 104.

As early as 1890 Stumpf pointed out a phenomenon which had hitherto not been mentioned by acousticians, and which seems in a measure to conflict with Ohm's law as interpreted by Helmholtz. "(*a*) If I take two tones about a semi-tone apart in the middle region of the scale (*e. g.*, g^1 and a^1 on the violin)," says Stumpf, "I hear the two primary tones, but also, over and above these, a third tone which lies between them,

somewhat nearer the lower than the higher. This third tone has a very soft coloring, and with keen attention is localized within the ear; it is this tone which beats, while the primary tones remain constant. The two primary tones are, in my judgment, noticeably weakened,—more than is customary when two tones are sounded at the same time.

"(*b*) If I take tones that lie farther apart, in the same region of the scale (*e. g.*, g^1 and a^1), I do not hear any middle tone, but only two primaries; and these two seem themselves to beat. If, however, I turn the attention more particularly to one of them, this always seems to be the beating tone.

"(*c*) If, on the other hand, I take two tones that lie much nearer together than a musical semi-tone, so that they approximate the difference limen for simultaneous tones, I get one tone, and that beating. It is difficult to say whether it lies between the primaries."[21]

As is well known, Stumpf attempts to explain this phenomenon of the so-called intertone (Zwischenton)[22] on the basis of an assumed 'physiological accommodation.' On this principle all the fibers affected by a certain given tone mediate the sensation corresponding to that tone, and *not* each the tone of its own period. If this supposition be not made, we should expect to hear a number of tones of different intensities for each single pendular vibration. Now, he says, the intertone of case (*a*), described above, is produced in this way: The two vibrating sections of the basilar membrane, corresponding to the given tones, overlap. In this condition there will be one intermediate nerve fiber equally affected, and more intensively than others, by both forms of stimulation. This fiber will then mediate the sensation of the intertone. This fiber may be supposed to constrain the neighboring fibers or cells in the direction of its own specific energy. The nervous structures excited will then fall into three groups—the upper and the lower correspond to the primary tones; the intermediate, to the *intertone.* The outside tones will be weakened by the loss of the inner vibra-

[21] C. Stumpf, *Tonpsychologie,* II., 1890, 480–1, translated by Bentley and Titchener, *Am. Jour. of Psychol.,* XV., 1904, 66–7.

[22] This cannot be translated '*middle tone.*' That would confound it with Hermann's 'mittelton' which I have thus translated.

tions contributing to the intertone, and the interference of the two modes of stimulation will affect this tone and make it beat. In the case of (*b*) the overlapping is too slight for the production of an intertone. Where the primaries are very near together in pitch the three tones simply fuse together.

Ebbinghaus accepts Stumpf's description of the phenomena, and wrongly supposes[23] that an intertone, such as is described, is to be expected on the Helmholtzian view. "The *Zwischenton* is precisely what the Helmholtz theory does *not* explain; it is precisely what we should *not* expect from that theory."[24] But it is to be noted that Ebbinghaus himself does not attempt to explain how such a tone is produced. He *has* no explanation. His theory admits of none. We shall briefly consider his theory.

Helmholtz's theory cannot explain the beats of wide intervals, he says: it cannot explain the origin of 'interruption tones,' which tones he still considers, with Koenig and Hermann, to be subjective,[25] and to arise from a rapid 'beating' of the given tone. He objects, moreover, to an explanation of combination tones which makes these tones so different in origin from beats as Helmholtz conceived the matter. Hermann's modifications are unnecessarily complex,[26] so Ebbinghaus proposes the following.

The general theory of analysis as defined by Helmholtz is good. It is inconceivable, however, that the nervous elements are from the first as closely specialized as Helmholtz's theory supposes. We may conceive of the cells of the cochlear nerve as at first non-specialized. *Each* cell may be able to mediate *every* tone sensation. The close association of each cell with a resonator of a particular period, however, causes it gradually

[23] Ebbinghaus, *Grundzüge der Psychologie*, 1902, p. 317. All the page references to Ebbinghaus' *Psychologie* refer to the first edition. The second edition (1905), which I did not have when the above was written, contains nothing necessitating any changes in Ebbinghaus' theory as stated here. In the later edition Ebbinghaus has, however, left out his objection that Helmholtz's theory cannot explain the origin of 'interruption tones,' but he objects to Helmholtz's explanation of beats and some related phenomena, p. 334.

[24] Bentley and Titchener, *op. cit.*, p. 69.

[25] Ebbinghaus, *op. cit.*, pp. 312–3. But see note 23, above.

[26] *Ibid.*, note, p. 317.

to acquire a special physiological habit for a certain condition of stimulation. It comes to respond most strongly to the period of its own resonator, though it still *can* respond to other periods. These other periods of stimulation may come to it in two ways: (1) by the weak response of its own resonator to tones of nearly the same frequency as its own, (2) by partial vibration. 'Every single tone-wave striking the basilar membrane *sets into sympathetic vibration not only the fibers directly attuned to it, but also, to a certain extent, all those fibers which are attuned to* its harmonic undertones—these, of course, into partial vibration by the formation of nodal points.' If, *e. g.*, a tone of 600 vibrations is given, it will set into partial vibration the fibers corresponding to the periods 300, 200, 150, 120, etc. *Now, for Ebbinghaus, pitch is determined directly by the frequency of the stimulation;* hence *all* of these cells when stimulated together by partial vibrations will mediate the same tone (*e. g.*, the one corresponding to 600 in the above case). Each cell then comes to respond relatively easily to the octave of its own period, less easily to the twelfth, and so on.

Now, *beating* is explained on the principle of interference of vibrations in the basilar membrane fibers, as it was by Helmholtz. Each of two interfering tones is thus periodically strengthened and weakened. In case of wide intervals interference is possible through partial vibrations of fibers corresponding to harmonic undertones of the given tone. Thus the difficulty of Helmholtz's theory of beats is overcome. But now just how these beats are mediated (*i. e., what* fibers mediate them and *how*), is the question. The *bearers* of the beats, says Ebbinghaus,[27] are not the intermediate overlapping fibers, but those directly correlated with the stimulus rhythms. The tones mediated by these fibers are then made to fluctuate periodically in intensity. When fast enough these beats, as Koenig held, may be sensed as difference tones. Here then we seem to have the anomaly of two tones mediated by the same fiber or cell. Perhaps not, for the higher tone may be of a period too frequent for the natural period of the cell. Very rapid beating, as beats that are rapid enough to give rise

[27] *Ibid.*, p. 317.

to tones, must be mediated by fibers set into partial vibration; for in such cases fibers set into whole vibration will be too far apart to overlap. The beating in these cases may have a period more natural to the cell thus affected through partial vibration than is that of the objective tone. In such a case the tone that is mediated is a difference tone.[28]

Somewhat inconsistent with this view is the one expressed on the previous page of his book, where Ebbinghaus seems to take the view that beats are experienced immediately from the form of the objective complex wave. This is, of course, a different consideration altogether, and such response to non-pendular forms of the objective wave requires in the fibers a condition that is directly opposed to such delicate elasticity as partial vibration requires. The above explanation was on the basis of partial vibration by means of the formation of nodes. Yet Ebbinghaus says this: 'The above tone waves *a* and *b* will . . . interfere with each other, *i. e.*, the amplitudes of their individual vibrations, and therewith their intensities, will periodically strengthen and weaken. These fluctuations . . . differ very much according to the intensity and pitch relations of the two tones. Under certain conditions they have the rhythm $h - t$,[29] under others $2t - h$, and so on. The small structures of the basilar membrane vibrating sympathetically should not, however, be thought of as structures of the nature of tuning forks,[30] as has actually been done since the Helmholtzian theory. But, although they are set into vibration by external impulses only when these impulses in a measure correspond to their own vibration numbers, they have, doubtless, still only slightly elastic power and *can continue their movements no considerable time after the cessation of the objective impulse.* They can, therefore, on account of their acquired disposition, . . . not remain unaffected by external impulses of their own period; and, accordingly, the above mentioned amplitude

[28] *Ibid.*, p. 323.

[29] h = higher tone, t = lower tone.

[30] Yet on p. 321 he says as proof of the possibility of partial vibration of the basilar membrane fibers that 'the deeper fibers of the contra octave of the piano . . . produce their twelfth, yea even their fourteenth partial tone as a most splendid after-clang of the tone.'

fluctuations of the objective tone waves will be taken up more or less faithfully by the portions of the basilar membrane on the whole corresponding to them. If they recur relatively slowly we hear them as beats; in case of more rapid frequency, as rattling; with still greater frequency as roughness.'[31] And this roughness, of course, on greater frequency merges into a difference-tone.

There is, therefore, at the basis of Ebbinghaus's 'explanation' a fundamental conflict. However Ebbinghaus may attempt to explain the intertone, he must posit for interference a high degree of inelasticity of the basilar membrane fibers. Such an assumption, though, is directly contradictory to one of his main presuppositions, that of partial vibration, which requires highly elastic fibers in the cochlea. His assumption of partial vibration of the basilar membrane fibers for the explanation of beats of wide intervals, is likewise contradicted by that of inelasticity to explain how beats can give rise to difference tones.

We shall turn back now to the consideration of a few more recent experiments on the so-called interruption and the variation tones. It will be recalled that those writers—Koenig, Hermann, Wundt, Ebbinghaus—who had been offering theories to replace or to supplement the Ohm-Helmholtzian view had all conceived of the interruption tone as inexplicable on that view. It was to them an evidence that beats when frequent enough generate tones.

In 1901 K. L. Schaefer and Otto Abraham[32] took up the study of 'interruption tones' with a view to testing whether they existed objectively as pendular vibrations. To interrupt the tone they used the method employed by Dennert, that of stopping up certain holes of a siren. A large wooden disk 4 mm. thick and 15 cm. in diameter was used. It was perforated by a circle of holes 5 mm. in diameter. Of these 44 were tightly closed in such an order that 4 open holes alternated regularly with an equal number of closed ones. The disk was rotated with a constant speed by an electric motor and a cur-

[31] Ebbinghaus, *op. cit.*, p. 322.

[32] 'Studien über Unterbrechungstöne,' *Pflüger's Archiv*, LXXXIII., 1901, pp. 207–211.

rent of air was blown through a tube upon the circle of holes. A resonator was held to the circle of holes at the opposite side and connected with the ear of the observer. The experimenters found that whenever the 'interruption tone' corresponded to the pitch of the resonator it was very perceptibly reinforced. Professor Stumpf was present to witness the phenomenon and was convinced that the 'interruption tone' was objective. They used an ordinary cylindrical resonator for the purpose. When the vibration frequency of the interruption tone equaled 300 the experimenters reinforced the tone with a wooden resonator for the 300 fork. The first overtone of the 'interruption tone' was also reinforced, so they concluded that this tone is not simple but is a clang.

Koenig had also produced tones from disks with holes periodically variable in diameter. As the rotation increased the beats from these holes increased and finally produced a continuous tone. Dennert had obtained similar results. Now, Schaefer and Abraham also reinforced this sort of 'interruption tone' with resonators, thus proving that it, too, is objective. *The tone was unmistakably reinforced.*

These results, conclude the experimenters, prove that the so-called interruption tones, whether produced by actual periodic interruption or simply by periodic strengthening of a tone, are objective; *they correspond to actual pendular vibrations existing in the air.* Our perception, then, of these tones is in no way contradictory of Ohm's law. The tones exist as physical facts, which are perceived as any objective tone is perceived.

In another series of experiments[88] Schaefer and Abraham found that a disk with a circle of 60 holes varying periodically (five times) according to this scheme (where o = open hole, x = closed hole).

o o o o o o x o x x o x

also gave an 'interruption tone' reinforceable with a resonator. In this case they obtained, when the tone 60 corresponded to c^4,

[88] 'Studien über Unterbrechungstöne,' *Pflüger's Archiv,* LXXXV, 1901, pp. 536–42.

the 'interruption tone' $5 = f$. In four similar experiments they varied the scheme respectively as follows:

o o o o o o x x o o x x
o o o o o o x x x x x x
o o o o o o x o x o o x
o o o o o o x o o x o x,

and in each case got results similar to the above, *i. e.*, besides the tone 60 ($= c^4$) also the 'interruption tone' 5 ($= f$) which in every case was reinforced with a resonator.

Using a toothed siren, they obtained 'interruption tones' reinforceable to a still higher degree. The first siren of this kind that they used was 9.5 cm. in diameter and had on its edge 180 teeth equally distant from one another. When the disk was rotated the teeth were lightly touched with a visiting card. Now when every ninth tooth was removed they heard, besides the principal tone 9, also one corresponding to 1. This, as stated, was strengthened by means of resonators even more than was possible with interruption tones from perforated disks. Using a siren of 100 teeth, of which every fifth was filed down, they obtained, when the principal tone was c^4, an 'interruption tone' a^1. This also was strongly reinforced.

They decided now to study Hermann's *phase changing tones* in a similar way. They found in the laboratory two of Hermann's toothed disks arranged to reverse the phase of the tone 24 times in every 180 vibrations. On the one disk a tooth was missing between every seventh and eighth space, so that two spaces fell together; on the other a space was missing between every seventh and eigth tooth so that two teeth occurred together. Now in these cases, as Hermann had already observed,[84] besides the tone 180, the one corresponding to 24 was also plainly audible. This latter is the tone Hermann thought was caused by the periodic phase reversals. *But Schaefer and Abraham found that it was strongly reinforced by means of physical resonators.* Various other cases of the supposed phase changing tones, cases that had been studied both by Hermann and by Meyer, were tested with similar results. 'From these

[84] *Pflüger's Archiv*, LVI., 1894, p. 490 f.

results, which agree throughout, it may be concluded,' say Schaefer and Abraham, '*that the change of phase in and of itself gives no occasion for the generation of a particular sort of tone;* and that in those cases (first observed by Hermann) where a phase changing principal tone is accompanied by a second tone whose vibration-number corresponds to the number of phase reversals, the latter tone is to be regarded as a simple 'interruption tone' (dieser letztere als einfacher Unterbrechungston zu betrachten ist).

Later these men took up further experiments along the same line.[85] They used first a paste-board disk (Pappscheibe) 0.5 cm. thick with a simple row of 24 holes of 2 cm. diameter. The resonator, which was held close to the circle of holes as the disk rotated, was connected with the ears of the observer by means of rubber tubes (Hörschläuschen) so that a very slight reinforcement could be detected. It was found that when the disk was rotated *without blowing the holes* it produced a weak tone whose frequency corresponded to the number of holes that passed the mouth of the resonator per second. This tone was audible even when it was as low as the contra-A; sometimes it was perceptible even to contra-E. It was easily reinforced by resonators. To distinguish these tones from 'interruption tones' produced when a rotating disk interrupts a tone, they called them disk tones (Scheibentöne). They have always, of course, the same pitch as the 'interruption tone' obtained when the holes are blown upon through a tube.

The experimenters found that the *variation* tones (corresponding to $n + m$ and $n - m$, when n is a tone interrupted m times) are very perceptible when the tone of a loudly sounding fork is interrupted by a rotating disk. They decided to test for the objectivity of these tones. The disk used in this case was aluminium, and was 1 mm. thick. It contained a circle of 86 holes each 1 cm. in diameter. The disk was rotated sometimes by hand, sometimes by electricity. For the generation of the tone to be interrupted by the disk, they used principally the Bezold-Edelmann series of forks. By means

[85] K. L. Schaefer u. O Abraham, *Studien über Unterbrechungstöne, Pflüger's Archiv,* LXXXVIII., 1902, pp. 475–91.

of movable weights these forks can be made to give a continuous series of frequencies from the lowest audible tone to that of the tone a^2.

The variation tones heard in this series of experiments are given in a table, page 483. From this table it appears that they never heard both variation tones at the same time, though this is not stated explicitly in the report, I believe. The higher variation tone is in general more easily perceived than the lower. The latter was never heard with forks below f^1, and neither one was heard with forks below *e*. The tones heard were reinforced by several kinds of resonators. As proof that these variation tones reinforced were not ' disk ' tones the experimenters found that they were strengthened *only when the fork,* held at the opposite side of the disk, *was directly opposite the mouth of the resonator.*

In view of these facts Schaefer and Abraham suggest the hypothesis that the so-called interruption tone is really a difference tone of the intermitted tone and one or both of the variation tones. The formulæ

$$(m + n) - n = m$$
$$n - (n - m) = m$$

show that it may possibly be a resultant of the coincidence of two difference tones. This idea, as the experimenters acknowledge, is not entirely original with them. For before Koenig had interpreted it as an interruption tone, A. M. Mayer, hearing this low tone, suggested that it might be regarded as " a resultant sound formed by the union of the sound of the fork with the upper and the lower of the secondary sounds,"[36] *i. e.,* of the variation tones. Difference tones, it will be remembered, were then frequently called *resultant* tones.

Koenig[37] had remarked that ' interruption tones ' are weak with low forks, while with high loud forks, where the variation tones are scarcely, or not at all audible, they are very prominent. The observations of Schaefer and Abraham agree with the results of Koenig.[38] The intermittence tones for Koenig were

[36] *American Journal of Sciences and Arts,* CIX., 1875.
[37] *Quelques Expériences d'Acoustique,* pp. 138–140.
[38] K. L. Schaefer in *Nagel's Physiologie des Menschens,* III., 1905, p. 534.

very strong when the ratio of the frequency of interruption to the frequency of the given tone was 1:16 and 1:32, where the tones c^4 and c^5 were used. In these cases, as Schaefer and Abraham suggest, the variation tones (15, 17 and 31, 33) would lie so near the tone of the fork as to be difficult of discrimination from it. Moreover, the two primary tones in such cases would form intervals very favorable for the formation of loud difference tones. The experimenters never perceived the interruption tone with the fork c^2 as distinctly as Dennert[39] reports himself to have done. Their observations agree more closely with those reported by Koenig. 'The interruption tone was best obtained when forks of so high a pitch were used that the variation tones were hardly or not at all perceptible.'[40]

Even before Schaefer and Abraham published their experiments on the interruption tone, Zwaardemaker[41] described an experiment of his own, intended to throw light on this phenomenon. He purposely diverged from the ordinary method of interruption of the tone by means of the perforated disk, and connected a Blake microphone with a couple of Leclanche's elements in the primary coil of a small induction spool. The secondary circuit was opened and closed 64 times per second by means of an electrically driven tuning fork. In this secondary circuit was a telephone which was held to the observer's ear. If the circuit was closed and the tone of the fork conducted through a pipe to the microphone, it was plainly audible to the observer. Then when the tone was allowed to be interrupted 64 times per second a powerful interruption tone was heard, says Zwaardemaker. The interruptions themselves when no tone was conducted to the microphone made only an ill-defined noise. Zwaardemaker thinks that there is generated an objective vibration corresponding to the 'interruption tone,' and that it is a matter not for physiology but for physics to deal with these tones. He seems not to have tested for the objectivity of these tones, however.

[39] Dennert, *op. cit.*, 177 f.

[40] *Pflüger's Archiv*, LXXXVIII., pp. 486–7.

[41] H. Zwaardemaker, *Ueber Intermittenstöne.* Englemann, *Archiv f. Physiologie*, Sup. Bd., 1900, pp. 60–7.

The results obtained by Zwaardemaker, apparently reacting somewhat slowly on Schaefer and Abraham, called out from them an extended series of experiments published four years later.[42] They desired to test more accurately the intensity relations of the 'interruption tone' and of the principal tone, under conditions similar to those described by Zwaardemaker. In their experiments they were able to vary within wide limits the pitch of the principal tones and the frequency of interruption. They used tones of as high pitch as 4,800 vibrations. From more than 200 different tests in which the tones and the interruptions vary within wide limits, both absolutely and with respect to each other, they obtained the following results: The primary tone, which was heard very distinctly in the telephone when not interrupted, weakened very much or entirely disappeared as soon as the interruption process began. On the other hand, the more or less complicated clang appeared in its place. This latter clang contained one or two characteristic partial tones whose vibration numbers were dependent upon the pitch of the given principal tone and upon the frequency of the interruption. Under special conditions a confused tone was heard whose frequency equaled that of the interruption. These results seem to show that Zwaardemaker had not described the phenomena carefully enough. The tones obtained were in every case reinforced by resonators.[43]

Schaefer and Abraham conclude—as indeed Zwaardemaker, with far less support from experiment, had done—that the so-called interruption tone exists objectively as a pendular vibration capable of affecting physical resonators; that its explanation, therefore, is not a physiological problem. This tone consequently offers no difficulty to the Ohm-Helmholtzian law of tonal analysis. It is perceived upon the same principle as that upon which other objective tones are perceived.[44]

[42] 'Zur Lehre von den sogenannten Unterbrechungstönen,' *Drude's Annal. der Physik*, XIII., 1904, pp. 996–1009.

[43] *Ibid.*, p. 1000, and Nagel's *Physiologie*, III., 1905, p. 536.

[44] From their various statements they seem to consider the interruption tone as partly a 'disk tone' and partly a difference tone of two sets of primaries so closely connected as to make it exist objectively. Both of these constituents of the 'interruption tone' then are *objective*.

SECTION 5. Intensity Relations.

We come next to one of the most difficult of acoustical problems. It is the question of *intensity relations* which we must now consider. It is a long story, but we must be brief. We shall take up, in connection with this question, the other troublesome one as to *what combination tones are actually heard.* The two problems are closely allied. This is evident if we admit—what may become clear soon—that there are combination tones which are not heard under normal conditions.

The earlier writers paid but little direct attention to the intensity relations of combination tones. Until Helmholtz's time only three or four difference tones had been heard.[1] In Helmholtz's work it is not always clear whether he actually heard the tones of which he writes. In his *Sensations of Tone,* page 155, *e. g.,* he considers difference tones up to the sixth 'order,' inclusive. In one place he tells us explicitly that he heard, from siren tones, not only the first summation tones but also those represented by $2p + q$ and $2q + p$.[2] These last, he says, were very weak. That summation tones in general are weak, he states both as the result of actual experience and in connection with his theoretical deduction.[3] He states explicitly that 'multiple' combination tones cannot as a rule be distinctly heard, but that in certain cases they make themselves known by beating with other tones.[4]

In 1876 A. M. Mayer made the important discovery that sounds of considerable intensity, when heard by themselves, may be completely obliterated by lower sounds of sufficient intensity. On the other hand he found that "no sound even when very intense, can diminish or obliterate the sensation of a concurrent sound which is lower in pitch."[5] This phenomenon, now well known, affects not only Ohm's law but also some

[1] Cf. Hällström, *supra,* p. 3.

[2] *Suprà,* p. 23.

[3] *Sensations of Tone,* pp. 155–6; p. 413. Mathematics can, indeed, not yet be applied to the determination of the *relative intensities* of combination tones; it is of use, however, in the determination of the *frequencies* of such tones.

[4] *Ibid.,* p. 154 d.

[5] A. M. Mayer, 'Researches in Acoustics,' *Phil. Mag.,* 5th Series, II., 1876, pp. 500–7.

objections urged against Helmholtz's theory of combination tones.

We have already considered Koenig's results as to the question of what combination tones (beat-tones) are audible. While he did not raise the intensity question so directly as it has since been raised, he frequently spoke of the relative intensities of the 'beat-tones' and endeavored, it will be recalled, to explain the more easily perceptible summation tones as resulting from upper partials.[6] Later he altogether abandoned a belief in the existence of combination tones.

In 1894 A. M. Mayer reports an interesting experiment which he performed with bird-call whistles. These whistles gave tones beyond the upper limit of audibility: "With these whistles," he says, "beat-tones [*i. e.*, difference tones] have been obtained when the vibrations of either whistle alone were inaudible.

"Beat-tones," he adds, "have also been obtained by Dr. Koenig and myself in Paris with tuning forks whose frequencies surpass the limit of audibility. Dr. Koenig anticipated me in the production of these beat-tones by several months."[7]

Hermann in 1891 definitely raised against the Helmholtzian theory the objection not only that difference tones were in many cases heard louder than they should be according to that theory, but also that difference tones of the 'second order' were in some cases much louder than those of the first. He cites the case of the major third $c^2:e^2$ which gives the difference tone $g^1 (= 2c^2 - e^2)$ very distinctly. This one case alone, he says, is sufficient to disprove the validity of the Helmholtzian theory of combination tones.[8] Stumpf had indeed shortly before this observed the same phenomenon. He found that g^1 comes out more clearly when c^2 and e^2 are sounded in a pianissimo than it does when they are sounded loudly.[9]

[6] *Supra*, pp. 51 ff.

[7] Alfred M. Mayer, 'On the Production of Beat-tones from Two Vibrating Bodies whose Frequencies are so High as to be Separately Inaudible,' *Rep. of the Brit. Assoc. for the Adv. of Science*, 1894, p. 573.

[8] *Supra*, p. 69.

[9] *Tonpsychologie*, II., 1890, p. 249. It must be noted, however, that on louder sounding of $c^2:e^2$ the first difference tone becomes so loud as to interfere with *F* on the principle discovered by Mayer that lower tones may obliterate higher ones.

For the perception of difference tones, he concludes, it is not at all necessary, as Helmholtz had supposed, that the primaries be intense. Stumpf had not at this time abandoned the Helmholtzian theory. Indeed, as he suggests,[10] the second difference tone, according to Helmholtz's mathematical deduction, is not dependent upon the first, even though Helmholtz himself considered it so in the body of the *Tonempfindungen*.

Hermann was never able to hear summation tones. These according to Helmholtz's theory should be about equally strong with the difference tones, he says. Stumpf, rather inconsistently with the view he then held, was inclined to adopt the explanation of summation tones first offered by Röber, in 1856, and later suggested independently by Appunn to Preyer. This view, it will be recalled, regards the summation tones as difference tones of the second order, thus:

$$2h - (h - t) = h + t.$$

Stumpf says that series of observations on the harmonium, the siren, and different pipe instruments have led him to the conclusion that summation tones are strongest when there are strong over-tones present.

Since Meyer's theory arose largely from the fact that no other theory sufficiently explains certain of the phenomena under consideration, we may briefly consider it here.[11]

In his most recent statement,[12] Meyer divides combination tones into three classes, as follows:

1. Subjective.

2. Objective I., tones which arise under conditions represented by a harmonium or polyphonic siren, where there is a common wind chest.

[10] *Tonpsychologie*, II., note 3, p. 250.

[11] Professor Meyer informs me that he is about to publish, in English, a complete statement of his theory, so I shall not attempt a full description of it here. Since the note of the previous sentence was written the promised monograph has appeared. "An Introduction to the Mechanics of the Inner Ear," by Max Meyer, *The University of Missouri Studies*, Vol. II of the Science Series, No. 1. As the monograph is only an 'introduction' it does not enter as fully as was hoped into the questions of intensity relations. Moreover it is theoretical rather than experimental.

[12] Über Kombinations und Asymmetrietöne,' Drude's *Annal. d. Physik*, XII., 1903, pp. 89–92.

3. Objective II., tones that arise from the fact that an unsymmetrical body is forced to vibrate synchronously with two or more wave-series.

So far as I see, Meyer practically agrees with Helmholtz's explanation (with certain necessary modifications) of cases (2) and (3).[13]

He admits that summation tones are sometimes heard in these two cases but *not* in case (1). Both kinds of objective combination tones are of little interest to the psychologist, he says. Meyer's so-called wave-reduction theory, then, applies properly only to the group of combination tones usually called subjective. This group of subjective combination tones evidently includes the prominent combination tones which have no corresponding objective pendular vibrations,—those whose intensity Helmholtz's theory is supposed incapable of explaining. This makes possible a good deal of ambiguity as to which group shall claim certain tones ordinarily admitted to be 'subjective.' All weak tones which this theory cannot explain can easily be given over to Helmholtz, *i. e.*, to groups (2) and (3) of the divisions just given.

Meyer's theory, though it attempts to explain the origin of only the 'subjective,' combination tones, is, however, a theory of how the ear analyzes tonal clangs. All tones originating in the middle ear, or externally to the ear altogether, must come to the inner ear as 'objective.' They must, therefore, all be treated alike. The principle of his theory is this: When a wave impinges on the ear the movement of the stapes corresponds in general to the objective form of the wave, whether it is a complex wave or not. This produces certain forced movements of the whole organ of Corti and basilar membrane. The membrane is crowded downward near the stapes to make room for the liquid displaced by the inward bulging of the *fenestra.* The extension of the movement is, of course, dependent upon the force exerted against it and for the period of time that this force acts. Hence in the case of low tones it extends farther up the cochlea than in the case of high tones. In the former

[13] It must be noted, however, that he absolutely rejects Helmholtz's theory of resonators in the ear. The process of analysis of the tones that come into the inner ear is *entirely different* for the two theories.

case the inward bulging lasts longer. Now if a high tone and a low one operate at the same time the lower portion of the organ of Corti will evidently be stimulated more frequently than the portion farther up the cochlea, which is affected only by the less frequent vibration. Whenever the two wave-series act upon the *fenestra* in the same direction at the same time the resulting inward motion will be greatest and the stimulation of the organ of Corti will extend farthest up the cochlea. Different sections of the organ will consequently be stimulated with different periods of frequency according to the form of the complex wave that is affecting the ear. Now, *pitch is determined entirely by the frequency of the stimulation of the organ of Corti.* Each section of this organ, therefore, that is stimulated will mediate a tone corresponding to the frequency of its stimulation.

It is to be noted here that Meyer's theory affords an easy explanation of the fact, which probably no other existing merely physiological theory can explain; viz., that a high tone may be obliterated by a lower one, whereas the opposite is not true. A low tone though very weak cannot be obliterated by a higher tone, even of great intensity.

By a very laborious process, partly determination of the form of the objective wave, and partly mathematical calculation, Meyer has endeavored to find, in the case of several given intervals, what tones *ought* to be heard, and what should be the amplitude and frequency relations of them.[14]

The results are not very satisfactory. In the interval of the fifth, *e. g.*, where the primaries have equal given amplitudes, the calculated results would make the difference tone (1) most intense, the lower primary (2) next in intensity, and the upper tone (3) comparatively very weak. It is, of course, but just to say that the relative intensity of tones cannot be determined solely by the ratios of their amplitudes. In reply to the charge that the tones actually heard do not correspond exactly with his calculated results Meyer suggests that a consideration at the same time of *all* the various aspects of the complex process is impossible; that results can be obtained from the consideration of only one of the variables at a time, to the

[14] See especially *Zeitschr. f. Psychol.*, XL., 1896, 177 ff.; and XVI., 1898, 22 ff.

neglect of the others. Very well, Helmholtz's mathematical calculations are made under very similar conditions, as Helmholtz himself admits. Without considering then the question as to whether Meyer's theory is superfluous, and as to whether his tri-partite division of combination tones is unnatural—such questions will be more in place when we shall have seen his new book—let us consider his laws for subjective combination tones.[15]

SECTION 6. Meyer's Laws of Combination Tones.

These laws "do not express *all* the difference tones which one might possibly hear in every possible combination of objective tones, but merely those differences [*i. e.*, difference tones] which one is *most likely* to hear in those combinations which correspond to relatively simple ratios."

1. When the ratio of the vibration rates does not differ much from 1:1 (*e. g.*, 11:12 or 9911:9989) only one difference tone is heard. It is expressed by the formula $h - t$, where h is the higher tone and t is the lower. In this case a "mean" or "intertone" is also heard, as described by Stumpf.

2. When the ratio of the frequencies is of the form $n:n+1$ the difference tone corresponding to 1 (*i. e.*, $h - t$) is always strongest. A few of those also appear whose numbers correspond to $n - 1$, $n - 2$, etc.; *e. g.*, the tones 8:9 gives 1 and 7, 6, 5. If n is a rather small number, we really hear *all* the tones from n down to 1; *e. g.*, the tones 4:5 give 3, 2, 1.

3. Other ratios of small numbers, representing intervals less than an octave, give combination tones represented by $h - t$, $2t - h$, $2h - 3t$. If the interval is less than the fifth, $h - t$ is strongest; if it lies between the fifth and the octave, $2t - h$ is the strongest.

4. Intervals larger than the octave do *not* give the first difference tone ($h - t$) which would be between the primaries. As a rule only one difference tone is easily noticeable in these cases. It is found by taking the "*smallest* difference between the larger number of the ratio and any multiple of the smaller number," *e. g.*, the tones 4:11 give $3 \times 4 - 11 = 1$.

[15] Cf. 'Auditory Sensations in the Elementary Lab. Course,' *Am. Jour. of Psychol.*, XVI., 1905, 293–301; *Zeitschrift*, XVI., pp. 2–3.

Now, as is easily shown from objective curves, any change in relative intensity of the primaries will not only make a difference in the relative intensities of the difference tones, but may even result in the appearance of new difference tones, or in the disappearance of some such tones which before the change were audible. Empirical facts here agree with theory. But Meyer has grave difficulties to meet in such cases, as did Koenig also, from the fact that determinations from the objective wave-form frequently indicate that a primary tone should have far less intensity than one or more of the difference tones, when such is not actually the case in hearing.[1] Meyer, of course, appeals to complexity of variables.[2]

In view of difficulties of this kind it is questionable whether Meyer's theory is an improvement upon that of Helmholtz even with respect to the intensity difficulty. And this is the very thing Meyer's theory was devised primarily to explain. Certainly the burden of proof of the superiority in this respect is on the *new* theory. It is true that on account of the "piece-meal" way in which it has been written, Meyer's theory has often been unjustly criticized. It remains to be seen how it will acquit itself of these difficulties when it appears in a single book.

But there are yet other difficulties. Meyer's theory has no place for summation tones and for difference tones lying between the primaries. Krueger in a series of very careful experiments with well-trained subjects, has proved that both of these kinds of tones can actually be heard with suitable intervals. The perception of them of course requires careful dis-

[1] Cf. Meyer's own curves in *Zeitschrift für Psychol.*, XI.

[2] "I think it is often overlooked in discussions on this subject," says Lord Rayleigh, "that a difference tone is not a mere sensation but involves a *vibration* of definite amplitude and phase. The question at once arises, how is the phase determined? It would seem natural to suppose that the maximum swell of the beats corresponds to one or other extreme elongation of the difference-tone. . . . Again how is the amplitude determined? The tone certainly vanishes with either of the generators. From this it would seem to follow that its amplitude must be proportional to the product of the amplitudes of the generators, exactly as in Helmholtz's theory. If so, we come back to difference tones of the second order, and their asserted easy audibility from feeble generators is no more an objection to one theory than to the other." *Theory of Sound*, Vol. II., 1896, p. 462.

crimination. Meyer has indeed heard these intermediate difference tones himself (*e. g.*, 5 from the intervals 4:9 and 3:8).[3]

In his review[4] of Schaefer's theory of subjective combination tones, however, I understand him to say that *subjective* difference tones lying between the primaries do *not exist*,[5] and that subjective summation tones are explicable as difference tones. Perhaps the various apparent contradictions on these points are due to his use of the term 'subjective.' Whatever stand he may take as to subjective summation tones and intermediate difference tones, they can apparently not be accounted for on the principle of his theory.

A few years ago, Krueger,[6] 'independent of any bias as to theory,' undertook an extended investigation of the phenomena resulting from two simultaneously sounding tones of intervals varying from $n:n$ to $n:4n$. He used tuning forks with adjustable weights. The tones were conducted through pipes from the resonance boxes of the forks to an adjoining room where the observer was seated. Upper partials were eliminated with considerable success by means of a number of side pipes perpendicular to the main one. The length of each pipe was one-fourth that of the wave of the overtone which was thus to be eliminated by interference. The forks were actuated with as much uniformity as was possible. The ground tone was usually c^1 (256) or one of its octaves c^2 or c^3. 'Occasional trials with other ground tones lead to no perceptibly divergent results.'

Krueger had *nine well trained observers*, one of them being a violinist with very acute analytic powers for high tones. Occasionally Krueger himself served as observer.

The article is very long. Only a few of the most important of the results can be given here. Krueger divides the intervals studied into three periods. The first extends from $n:n$ to $n:2n$, the second from $n:2n$ to $n:3n$, the third from $n:3n$ to $n:4n$.

Summation tones were studied only incidentally, *i. e.*, the

[3] *Zeitschrift für Psychol.*, XI., pp. 186–7.

[4] *Pflüger's Archiv.*, LXXXI., 1900, p. 56.

[5] Kreuger so understands him too. Cf. *Phil. Stud.*, XVII., 1901, p. 205 note.

[6] Felix Krueger, 'Beobachtungen an Zweiklängen,' *Phil. Stud.*, XVI., 1900, pp. 307–379 and 568–663.

observers were not asked to study them, but frequently called attention to them. They were heard, however, in all three periods, but were loudest in the first where the interval is less than an octave. The violinist who served as subject, was able to hear them at almost any time. The most favorable interval for the hearing of summation tones proved to be in the proximity of the major seventh. Krueger feels sure that these tones are really *summation* tones, for the primaries proved to be practically free from overtones. The pitch was determined by means of a tonometer. Occasionally (as was found to be the case with difference tones as well) the summation tones would seem to be 'subjectively raised or lowered.' In the third period (intervals between $n:3n$ and $n:4n$) the summation tones were too high for the tonometer, and hence could not be accurately measured. Dr. Möbius, the violinist, frequently called attention to them even here. Summation tones as a rule appeared towards the end of the clang as if in the period of loudest sounding of difference tones they had been obliterated.

Low difference tones were, in general, heard earlier in the clang, and for a shorter time, than higher ones. In both of the periods in which the intervals were greater than an octave (*i. e.*, $n:2n$ to $3n$ and $n:3n$ to $4n$), the first difference tone was heard. This difference tone, it will be noted, was *intermediate in pitch between the primaries.* Stumpf has recently acknowledged that he is convinced of the existence of intermediate difference tones.[7] These tones are always very weak in intensity. The results of Krueger's experiments show that from the compound clang of two simple tones there result, besides the summation tone, about five difference tones of different orders whose pitches may be determined from the following rule: Find first the difference of the vibration numbers of the primaries; then continue to find the difference between the two smallest numbers resulting after each successive subtraction. The series of differences obtained represents the difference tones.[8] Representing the first difference tone by D_1, the second by D_2, etc., Krueger gives the following formulæ for the determination of the pitch of all but D_5:

[7] *Zeitschr. f. Psychol.*, XXXIX., 1905, p. 268.

[8] F. Krueger, *op. cit.*, pp. 22–3.

$D_1 = n' - n$ (n' being the higher primary tone).

$D_2 = \pm (n - D_1)$.

For intervals smaller than the octave:

$D_2 = n - D_1 = 2n - n'$.

$D_3 = \pm (D_2 - D_1) = \pm (3n - 2n')$; or

$= D_2 - D_1$, when the interval is less than the fifth, and

$= D_1 - D_2$, when the interval is between the fifth and the octave.

$D_4 = \pm (D_3 - D_1) = \pm (4n - 3n')$ for intervals up to the fifth; or

$= D_3 - D_1$ for intervals smaller than the fourth, and

$= D_1 - D_3$ for intervals between the fourth and fifth.

$= \pm (D_2 - D_3) = \pm (4n - 3n')$ between the fifth and the octave, or

$= D_2 - D_3$ for intervals between the fifth and the major sixth, and $D_3 - D_2$ for intervals between the major sixth and the octave.[9] In no case was the difference tone $2D_1$ of the first over-tone heard.[10]

Intertones (Zwischentöne) were frequently observed. This phenomenon occurred not only between primary tones of small intervals but also between a primary and a difference tone, or between two difference tones. The intertone, as Krueger describes it, does not seem to be so definitely and distinctly a *tone* as one would suppose from Stumpf's description.[11]

Helmholtz's theory had been objected to on account of the fact that it could not explain the beats of wide intervals, such as Koenig had observed. While Stumpf early observed these phenomena which Koenig had described, he remarked that there are two kinds of beats easily distinguishable to the practiced ear,—beats of the higher primary tone, and deeper beats connected with the lower tone.[12] When overtones of the pri-

[9] *Ibid.*, pp. 326–7.

[10] *Phil. Stud.*, XVII., 1901, p. 222.

[11] In my own introspections it is always, more or less, a beating mass and the primary tones are drawn somewhat nearer together. If one of the primaries is suddenly stopped, the other at once makes a little 'jump' to its normal pitch. Mr. Bingham, who has a well-trained ear, tells me that his experience is similar to mine in this respect.

[12] C. Stumpf, 'Ueber die Ermittel. von Obertönen,' *Wied. Annal.*, LVII., 1896, p. 660 ff.

maries were present both kinds of beats were perceptible, but when, by means of interference, the upper partials were eliminated the beating of the upper primary tone disappeared while that of the *lower tone remained unaltered.* Stumpf explained the upper beating as due to interference of overtones of the lower tone with the upper primary, and the lower beating he supposed to arise from the interference of the lower primary tone with a difference tone of nearly the same pitch. Meyer reports that both these sets of beating (*i. e.,* upper and lower) disappear when the first overtone of the lower primary was eliminated through interference.[13] Meyer says his tones were weakened considerably in passing through the interference pipes for the elimination of the overtones. Krueger suggests[14] that this probably explains the difference. Meyer's primaries were weakened so much that difference tones did not exist. Krueger, from his extended experiments with tones freed from upper partials by a better interference method, finds it possible to account for all the beats of wide intervals, not due to overtones, by the presence of difference tones which either beat with the lower primary tone or with each other. As upper partials were gradually removed by interference, he found that the beating with the upper primary gradually disappeared. Krueger thinks that his experiments remove from the 'beat-tone' theory its most valuable support.[15]

Krueger thinks that his results indicate that there are no subjective overtones, such as might be expected from Helmholtz's mathematical theory. This, it seems to me, however, cannot be urged with much force. These overtones would be very weak and would be hard to discriminate from the generating tones (an octave below).

While the Helmholtzian theory affords an easy and simple explanation of various pathological cases, which need not be described here, it is well known that it has proved insufficient for the explanation of the so-called subjective combination tones. Dennert[16] and others have reported cases in which such

[13] *Zeitschr. f. Psychol.,* XVI., 1898, 9 f.

[14] *Phil. Stud.,* XVI., p. 233.

[15] *Ibid.,* p. 246.

[16] *Arch. f. Ohrenkeilk.,* XXIV., 1887; also Nagel's *Physiol. des Menschen,* III., 1905, p. 569.

combination tones have been heard by subjects who had lost from both ears the tympanic membrane and the first two ossicles. I have myself, with Mr. Bingham,[17] studied such a case in which the subject readily heard both the *first* and the *second* difference tones of tuning forks. While it is true that cases of this kind do not prove that Helmholtz's theory is *wrong*, they, at any rate, show that subjective combination tones may be due to causes other than asymmetry in the drum. Helmholtz's explanation of the origin of such tones is, to say the least, incomplete.

In recent years K. L. Schaefer[18] has proposed a supplement to the Helmholtzian theory of subjective combination tones. Helmholtz had made a very simple mathematical statement concerning the origin of *objective* combination tones when the primary tones were produced by generators with a common windchest (*e. g.*, the polyphonic siren). He admitted that his treatment of the case was very imperfect.[19]

A complete statement of the conditions would give more resultant tones than those obtained. The essential condition for the generation of such resultant objective tones is this. There must be exerted on the air escaping through one of the periodically opening and closing holes of the wind chest, a periodic change in pressure due to the escape of air through the other hole. Now Schaefer sees in the inner ear a condition fulfilling this requirement and analogous to the case of the siren, when the tones *m* and *n* fall on the ear the movement of the stapes against the oval window may, for practical purposes, he argues, be conceived as equal to two such organs operating separately against that window. Conditions similar to those in the wind chest of the polyphonic siren will then be produced in the liquid of the inner ear. There will be a periodic amplitude fluctuation of the 'vibrating bodies' set into motion by the primary tones. This will give rise to the combination tones

[17] Cf. W. D. Bingham, 'Rôle of the Tympanic Mechanism in Audition,' *Psychol. Rev.*, XIV., 1907, p. 229.

[18] K. L. Schaefer, 'Eine neue Erklärung der subj. Combinationstöne auf Grund d. Helmholtz'schen Resonanzhypothese,' *Pflüger's Archiv*, LXVIII., 1899, pp. 505–526.

[19] Cf. *supra*, p. 21.

$n + m$, $n - m$, etc., as is shown by Helmholtz's determination of objective combination tones.[20] As evidence in favor of his view he cites the rather close correspondence between the objective and subjective combination tones. He himself undertook an investigation of the first difference tone with intervals greater than an octave. Besides other instruments, he used for generating the primary tones both tuning forks and the harmonium; *i. e.*, he tested for the existence of both subjective and objective intermediate (zwischenliegenden) difference tones. In no cases were they audible. Even properly tuned resonators did not make audible the *objective* difference tones in question. He concludes from his experiments '*that both subjective and objective intermediate difference tones either do not exist at all, or that, as opposed to other difference tones, they are at least too weak to be perceived under the ordinary conditions of hearing.*'[21] Koenig and Meyer had heard difference tones intermediate in pitch between the primaries.[22] This Schaefer does not deny. He explains them, however, as being due to upper partials thus:

$$8 - 3\ [= 7 \times 3 - 2 \times 8] = 5.$$

He admits on page 520, that he himself with Professor Stumpf had heard the tone 3 as a difference tone from the interval 5:2. This tone, he says, is accounted for on the same principle, *i. e.*, $4 \times 2 - 5 = 3$. Summation tones when they have been heard, are to be accounted for also as difference tones resulting from the presence of upper partials.

Max Meyer in a review of Schaefer's article,[23] denies the alleged correspondence between objective and subjective combination tones. Rücker and Edser, he points out, had proved the objective existence both of summation tones and of inter-

[20] Recently Schaefer reports an experiment (in *Drude's Annal. d. Physik*, XVII., 1905, p. 572 ff.) in which he demonstrated that membranes vibrating to two tones give rise to objective combination tones. It is not clear whether he means to apply this to the inner membranes of the ear, as part of his explanation of subjective combination tones.

[21] *Pflüger's Archiv*, LXVIII., p. 512.

[22] Cf. *Pogg. Annal.*, CLVII., 1875, p. 194, also 216; *Zeitschr. f. Psychol.*, XI., 1896, pp. 186–7.

[23] *Pflüger's Archiv.*, LXXXI., 1900, 49 ff.

mediate difference tones when the primaries are produced from generators with a common wind chest. On the other hand, he says, the corresponding subjective combination tones do not exist (?). Meyer has elsewhere[24] expressed the conviction that subjective intermediate tones *are* audible. He thinks now that he was wrong.

Meyer ridicules the idea of analogy between the ear and the wind chest of the harmonium or siren. 'The Helmholtzian formula,' he says, 'is applied to a case here to which it is absolutely inapplicable (den sie absolut nicht passt).'[25] As to vibrating bodies in the air (from which Schaefer had drawn an analogy), Meyer says, 'Neither any theory nor any experience establishes the assumption that a body vibrating in a liquid (Flussigkeit) with the simple tone *m*, is in any perceptible way influenced by the pressure fluctuations due to the tone *n* in the surrounding medium. Much more, the general and well grounded conviction exists, that two bodies vibrating in the same fluid under normal conditions remain mutually uninfluenced.'[26] In his later article in the *Anal. d. Physik*, referred to above, Meyer mentions the experiment of Rücker and Edser as a positive proof against such an assumption. These experimenters, however, state explicitly that they lay 'less stress' on their negative than on their positive results. The resonating fork and mirror may not have been delicate enough for the detection of weak vibrations that may actually have been present. Of course Meyer's phrase "in any perceptible way" saves him here, *so far as experiments have gone.* Meyer's statement concerning theory, however, should be compared with the quotation from Lord Rayleigh on p. 65 above.

In reply to Meyer's review, Schaefer[27] admits that objective summation tones and subjective intermediate difference tones exist, but says that they are too weak to be heard under normal experimental conditions (üblichen Versuchsbedingungen). He

[24] 'Ueber Combinationstöne, etc.,' *Inaug.-Dissert.*, Berlin, 1896, p. 12.

[25] *Pflüger's Archiv*, LXXXI., p. 49.

[26] *Ibid.*, p. 54.

[27] *Pflüger's Archiv*, LXXXIII., 1901, 73 ff.

says also that the same thing is true of the corresponding subjective tones.[28]

We shall now consider the question whether the conditions of origin of *objective* and *subjective* combination tones are not really *in principle* the same, even though they are conveniently treated mathematically as different. For purposes of brevity we shall speak of the case of *objective* combination tones as case I., and of that of *subjective* combination tones as case II.

Helmholtz makes some statements that will be of use to us in this connection. (1) He admits that he has considered case I. only in its simplest aspect; that the complete development is very complicated and will result in the determination of combination tones of various 'orders';[29] and he tells us that he himself has heard even summation tones of the 'second order' from primaries generated with the siren.[30] (2) He reports, as the result of experiment, that even these supposedly objective combination tones are largely generated within the ear itself.[31] We shall see later that as carefully gathered empirical facts accumulate, the objective and subjective combination tones come more and more to a correspondence with one another. (3) It is admitted by Helmholtz,[32] and also by Lord Rayleigh, one of the best authorities on sound waves, that the motions of air and other elastic media admit of a treatment perfectly similar to that of case II., as found in Appendix XII., *Sensations of Tone*. In a reply to an objection urged by Hermann,[33] that Helmholtz's explanation of subjective combination tones depends entirely upon an assumed failure of symmetry, Lord Rayleigh says: "This objection . . . is of little practical importance, because the failure of symmetry nearly always occurs. It may suffice to instance the all important case of aerial vibrations. Whether we are considering progressive waves advancing from a source, or the stationary vibration of a resonator,

[28] Krueger's experiments proving that they are actually audible to trained observers, had already been published. *Phil. Studien*, XVI., 1900, p. 307 ff.

[29] Helmholtz, *op. cit.*, p. 420.

[30] *Supra*, p. 23.

[31] *Supra*, p. 21.

[32] *Helmholtz*, op. cit., p. 412 a.

[33] *Pflüger's Archiv*, XLIX., 1891, p. 507.

there is an essential want of symmetry between condensation and rarefaction, and the formation in some degree of octaves and combination tones is a mathematical necessity."[34]

This statement makes it all the more probable that the two cases developed by Helmholtz can both be worked out on the same principle of 'superposition of vibrations.' It seems to me that Helmholtz is not right in supposing that objective combination tones can be deduced from the siren even where the vibrations are *infinitely* small,[35] for, as he himself explicitly states, no combination tones will arise until there is "a second greater opening of variable size, through which there is a sufficient escape of air to render the pressure *p* periodically variable, instead of being constant." For the principle of superposition to take effect it is necessary only that the generating tones should somewhere be closely associated, and that at this place, wherever it happens to be, the amplitudes of their vibrations should have a finite ratio to the mass vibrating in common with the two tones. Now where such connection does not obtain externally to the ear, the conditions certainly are fulfilled *within* the ear. Even though we exclude all considerations of the membranes themselves, we find a favorable condition in the *fluids of the cochlea.* The objection that in these fluids the vibrations are very small is easily met by the fact that the mass of the vibrating structure (*i. e.*, the fluids) is also very small, so that the proportion still may easily be finite.

It is gratifying to note that this view is by no means contradictory to what Lord Rayleigh has to say. To quote: "The production of external or objective combination-tones demands the coexistence of the generators at a place where they are strong. [He adds in a footnote: 'The estimates for condensation of sounds just audible make it highly improbable that the principle of superposition could fail to apply to sounds of that order of magnitude.'] This will usually occur only when the generating sounds are closely associated as in the polyphonic siren and in the harmonium. In these cases the conditions are especially favorable, because the limited mass of air included

[34] Rayleigh, *Theory of Sound,* Vol. II., 1896, p. 459.

[35] See quotation from him, *supra,* p. 19.

in the instrument is necessarily affected by both tones,"[36] which is, of course, equivalent to saying that the proportion of the amplitude to the mass affected is a *finite* one.

Whether or not the two cases are physically one in principal—and we shall leave this question here with physicists—we may still, on the authority of the statements of Helmholtz and of Lord Rayleigh just referred to, apply the principle of superposition to the liquids of the inner ear, where the vibrations of the primary tones are in close relation. We may suppose that all subjective combination tones arise by this means. Let us see then what combination tones *might* be heard according to Helmholtz's determination given in Appendix XII., pp. 412–3, of *Sensations of Tone*. The second term (x_2) of the series for x gives

$$2p, \; 2q, \; p - q, \text{ and } p + q.$$

The third term (x_3) gives

$$3p, \; 3q, \; 2p \pm q, \; p \pm 2q.$$

This is as far as Helmholtz has carried the deduction. When it is carried farther the fourth term (x_4) gives

$$4p, \; 4q, \; 3p \pm q, \; 2p \pm 2q, \; p \pm 3q.$$

The fifth term (x_5) gives

$$5p, \; 5q, \; 4p \pm q, \; 3p \pm 2q, \; 2p \pm 3q, \; p \pm 4q;$$

and so on. In general the ith term (x_i) gives

$$ip, \; iq, \; (i-1)p \pm q, \; (i-2)p \pm 2q, \; (i-3)p \pm 3q \; \ldots \; 2p \pm (i-2)q, \; p \pm (i-1)q.$$[37]

Helmholtz's assumption upon which he based his equation of motion is of course only an arbitrary one, but the results show that the hypothesis which explains combination tones as resulting from superposition of the primary tones accounts for tones which *may* have occasionally been heard by Koenig and others and which have been regarded by some of Helmholtz's opponents as inexplicable on the basis of his mathematical

[36] Lord Rayleigh, *Theory of Sound*, II., 1896, p. 459.

[37] I am indebted to Professor F. R. Moulton, of the University of Chicago, for the further integration of the Helmholtzian equation.

theory. It is, of course, to be expected that only a few of all of these resultant tones are at any time audible and that those heard will belong to the 'lower orders.' The fact that occasionally a second difference tone $(2q - p)$, *e. g.*, is experienced louder than a first ought not to be urged too strenuously against Helmholtz's theory, not, at any rate, until we know more of the exact structures in the inner ear which are concerned in this purely mathematico-physical statement. Periods within the most practiced or most usual range, *e. g.*, ought perhaps to be experienced as relatively louder than periods less frequently experienced. The second difference tone of the major third (4:5), *e. g.*, is considerably louder than the first *only when its pitch is near the middle of the ordinary scale.* This is at least true of the forks at my command. Pure mathematical treatment can be applied to the operation of anatomical structures only with caution. Any criticism of Helmholtz's theory of hearing, then, which is based on the failure of the theory to explain the intensity relations of combination tones as actually experienced must take account of such obvious facts as those here indicated.

When the vibrations are so large that the displacements affect the *fourth* power of x in the equation $k = ax + bx^2 + cx^3 + dx^4$, another series of tones will arise, some of which will coincide with some of those determined above where only the *second* power of x was considered.

PART II. EXPERIMENTAL.

The Explanation of Summation Tones as Difference Tones of Upper Partials Shown Impossible.

Wherever it has been practicable I have repeated the experiments reviewed in the foregoing part of the paper, not so much to verify the results—my purpose was not so pretentious—but to make more real to myself the conditions and phenomena under consideration.

I have not been able to hear Hermann's phase-changing 'middle tone.' The intertone described by Stumpf and others is to me not really a tone; it is more nearly a beating complex, involving, for small intervals, both of the primary tones. In such cases, when one of the primaries is suddenly stopped, the pitch of the beating complex makes a clearly perceptible shift to the pitch of the remaining tone. The intertone is by no means so clear to me as one would expect from Stumpf's description of it.

By putting a loop of a string round the stem of a tuning fork and tying the ends to two hooks in the ceiling, one can easily illustrate the beating phenomenon of the rotating tuning fork. In such a case the fork can be 'wound up' like a top and left to unwind while sounding. The beats are then clearly perceptible. By pulling on the strings one can increase the rate of rotation until the beats become very rapid. The easiest way, however, of producing the 'interruption tone' is probably to stop up the holes in an ordinary metallic siren-disk so that about three open holes alternate with three closed holes, and to blow through a tube upon these holes while the disk is being rotated. This, it will be recalled, was Dennert's method. To me the 'interruption tone' is usually more or less ill-defined. When the rate of rotation of the disk is very rapid this tone becomes less easily perceptible. This may be due to the noise of rapid rotation, a disturbance that is practically unnoticeable

on slower rotation. At a certain medium speed of rotation the ' interruption tone ' is relatively more prominent. The intermitted tone is clearly audible even on very rapid rotation. This tone was always easy to locate because it was produced from one of the four circles of holes in the disk representing a major chord. A good way to hear what K. L. Schaefer calls the *disk tone* is this: Rotate rapidly a metallic disk perforated with a circle of large holes, holding the ear near the circle. Now and then touch the circle of holes with the corner of a piece of paper. The paper is, of course, set into vibration and produces a tone equal in pitch to the ' disk tone.' The former serves to locate the latter. The ' disk tone,' I found, is audible to the unaided ear. It is this tone which Schaefer regards as partly constituting the so-called interruption tone. I produced good ' interruption tones ' with paste-board disks perforated with a circle of holes varying periodically in diameter, by blowing upon the holes (while the disk was in rotation) through a flat tube as wide as the greatest diameter of the holes. The ' interruption tone ' thus produced is doubtless purely objective, as Schaefer has shown, the increase in the size of the holes being practically equal to an addition of other circles of holes of equal period as illustrated in the accompanying figure.

o O o O o O o O

A

o o o o o o o o

o o o o

B

It is possible that a disk perforated with a single row of holes periodically variable as shown in *A* will produce two tones similar to those of a disk with two circles of holes as represented in *B*, when both circles of the latter are blown upon through separate tubes.

I did not attempt to test the objectivity of the ' interruption tones,' feeling that I had not sufficient control of the speed to locate the pitch satisfactorily.

The experiment of Cross and Goodwin is easily repeated. I put a small piece of wax into each ear and touched each piece

with the stem of a vibrating tuning fork. When the forks were of nearly equal pitch the beats were clearly audible, even when the tones were very weak. The shift in the apparent direction of the sound, described by Lord Rayleigh,[1] seemed to me to be rather one of *emphasis,* or intensity, both tones being heard in their proper locations continuously. After the tones could no longer be heard to beat when the forks were held against the top of the head, or one against the wax in one ear and the other against the teeth, they were still heard to beat when held against the wax one in each ear. It is interesting to note, in this connection, that it is practically impossible for a low tone to obliterate a higher one when the tones are thus communicated separately through the wax in the ears. This result, if my observations are substantiated by other tests, is most easily explicable on the basis of a theory like Meyer's or that of Kuile's.

In my own case it took considerable practice to hear intermediate difference tones and summation tones from tuning forks, but I feel satisfied that I succeeded in both cases. Some of the other students in the laboratory heard the summation tone after very little practice.[2]

The important experiments *not* repeated in this work are (1) those, as has just been said, which were devised to prove the objectivity of 'interruption tones' (Schaefer and Abraham); (2) those devised to produce 'phase-changing tones' (Koenig, Hermann, *et al.*) and those which proved that these tones are strengthened by physical resonators (Schaefer and Abraham); (3) those experiments which have established with certainty the objectivity of combination tones in cases where the primary tones are in close mechanical connection externally to the ear (Rücker and Edser, *et al.*); (4) elaborate tests on a great number of intervals—both consonances and dissonances—to determine the general laws for the occurrence of combination tones (Koenig, Meyer, Krueger) and (5) experiments, not yet carried out satisfactorily by any one, studying the intensity relations of combination tones.

[1] Note, p. 46, *supra.*

[2] See below, p. 125.

The historical treatment has been made as brief as it was thought well to make it in view of the fact that no adequate statement of results already obtained has hitherto been given in the English language. The experiments which follow are limited to the investigation of those explanations of the summation tone which make it simply a difference tone of some sort. In this limitation, the fact has not been overlooked that the problem of intensity relations is probably the most important of all the present day open questions in the field of acoustics. For two good reasons this problem is not taken up here. (1) The (Koenig) tuning forks of this laboratory, and possibly of any other laboratory, are inadequate for extended experiments bearing upon this question. Not only are they not variable in pitch to any extent, but different forks vary greatly in the quality (in the non-technical sense) of their tones.[3] And this is not all. Some of the forks seem to be stiffer and less easily actuated than others, and their tones disappear earlier. It is practically impossible, therefore, to secure any uniformity in the primary tones used. The various 'wind-instruments' are even more unsatisfactory in experiments on intensity. With such instruments it is next to impossible to get smooth feeble tones. (2) Max Meyer is about to publish, in English, a complete statement and justification of his own theory of hearing.[3a] This theory was devised principally to explain the intensity relations of combination tones where Helmholtz's theory is said to fall short. It is but fair, then, to leave to Meyer the intensity problem. This second reason also explains why, in the historical sketch, the question of intensity relations did not receive as full treatment as it merited.

The method followed in these experiments is largely that of detecting feeble tones by means of 'auxiliary tones,' as they were called by Koenig. This method was used with considerable confidence by Koenig,[4] and has also been used to some extent by Lord Rayleigh.[5] It seems to be discredited by Krueger

[3] See Dr. A. Wyczolkowska, 'A Study of Certain Phenomena Concerning the Limit of Beats,' *Psy. Rev.*, 1906, XIV., p. 378.

[3a] Cf. note 11, p. 91, above.

[4] See quotation, *supra*, p. 52.

[5] *Theory of Sound*, Vol. II.

who says that he 'never reverted to the deceptive method of beating auxiliary forks.'[6] This statement, it is true, is made in connection with the study of *intermediate difference tones.* In such cases the method is unquestionably useless, and even deceptive. *E. g.*, if the auxiliary tone 5^{-} [7] beats when the primaries 4 and 9 are given, we are not at all sure that the beats indicate the presence of a difference tone 5. As some of the experiments which follow suggest, we are, in this case, more likely to hear beats of a difference tone 4^{+} (*i. e.*, $9 - 5^{-}$) with the lower primary. Koenig's experiments with auxiliary forks are subject to criticism in this respect. In the following experiments, where the details of the methods used are explained more fully, the possibility of error here indicated, as well as others that will become evident, are carefully guarded against. There certainly are legitimate uses of auxiliary tones.

It is well known that when two or three given tones produce two difference tones of nearly the same pitch, both or all three (as the case may be) of the generating tones beat very plainly. The beating is in general most prominent when the interfering difference tones are both of the so-called first order. Krueger[8] lays much stress on the fact that, when upper partials are ruled out, the beating of mistuned consonances is due to the interference of low difference tones. Of course the primary tones may themselves beat when the interval is small. In the cases of imperfect consonances, however, one, at least, of the difference tones which interfere must be of an 'order' higher than that of the first. Suppose that the frequencies are 200 and 301. Here the first difference tone is $301 - 200 = 101$. The second is $2 \times 200 - 301 = 99$. Now $101 - 99 = 2$. Consequently two beats per second would be heard. When an interval like this is sounding so feebly that the difference tones are not heard, one invariably locates the beating in the *primary* tones. Even when the first difference tone of the imperfect fifth is plainly audible I can never locate the beating solely in this tone. The primaries themselves always beat for

[6] *Phil. Studien*, XVII., p. 210.

[7] The mark ($^{-}$) indicates that the tone is slightly depressed, ($^{+}$) that it is slightly raised.

[8] *Phil. Stud.*, XVII., 1901.

me. Now when *three* generating tones are given in such ratio that their two *first* difference tones interfere, these primary tones are all heard to beat very plainly. Here are some intervals that I have found very good to illustrate this point:

$Ut_8 : Mi_8 : Sol_8^{-}$[9] $(4:5:6^{-}, 5 - 4 = 1$ beats with $6^{-} - 5 = 1^{-})$,

$Ut_4 : Mi_4 : Sol_4^{-}$ $(4:5:6^{-})$,

$Ut_8^{-} : Sol_8 : Ut_4$ $(2^{-}:3:4)$, also the octave of these.

$Sol_8 : Mi_4 : 7^{-}$[10] $(3:5:7)$.

Several other intervals as good for this purpose might be added. In some cases a second difference tone may beat with a first, *e. g.*, $Ut_4 : Mi_4 : 7^{-}$ $(4:5:7^{-})$. Here $2 \times 4 - 5 = 3$; $7^{-} - 4 = 3^{-}$.

Lord Kelvin,[11] indeed, pointed out this fact as early as 1878. He "found that the beats of an imperfectly tuned chord 3:4:5 were sometimes the very last sound heard, as the vibration of the forks died down, when the intensities of the three sounds chanced at the end to be suitably proportioned." When intervals of this kind are sounding loudly the *difference tones* are easily heard beating; but the primaries and the difference tones are so closely interconnected that the whole system of tones beats beautifully.

This last statement is less true when three primary tones are given in such relations that one of these tones beats with a difference or summation tone of the other two. Suppose, *e. g.*, that Ut_4 and Sol_4 are sounding loudly, and that while one hears these tones one holds Mi_5^{-} with a very feeble tone to one's ear. In such a case the tone Mi_5^{-} beats very plainly. In cases where the so-called auxiliary tone is very weak I simply hear a beating and no tone. The beating is then located distinctly at the pitch of the auxiliary. Only on very careful attention (and for me with the auxiliary fork sounding so loudly that it is audible as a tone) is beating also located in the other primary tones. The beating of the two lower primary tones, when heard, is always of the same frequency as that (louder beating) of the auxiliary

[9] The mark (⁻) indicates, as has been said, that the note is slightly flattened, by putting a piece of wax on one of the prongs. It is immaterial which note is flattened, except that high forks when thus weighted do not vibrate long.

[10] This is the 7th partial of Ut_1.

[11] *Proc. Roy. Soc. of Edin.*, IX., 1878, p. 602, cited by Rayleigh, *op. cit.*, p. 467.

tone. The forks may be interchanged and one of the lower ones used as the 'auxiliary' and, therefore, sounded more feebly and held to the ear. The results are much the same, *i. e.*, all three tones may still be heard to beat; but the highest tone, within certain limits, always, for me, beats most distinctly. Rapid beats especially are more easily heard with high tones than with low ones. This, of course, would be expected from Helmholtz's explanation of beats.

What was said in the last paragraph about locating beats also in the lower tones, applies especially to tones of such relations that the interval between the middle of three primaries and the upper tone (the auxiliary sounding feebly at the ear) is considerably smaller than an octave. The following intervals are illustrative, and were used in the experiment:

Interval	Auxiliary Fork
$Fa_3 : La_3$ (4:5)	Sol_4^- (9^-)
$Ut_4 : Mi_4$ (4:5)	Re_5^-[12] (9^-)
$Ut_3 : Sol_3$ (2:3)	Mi_4^- (5^-)
$Re_3b : La_3b$ (2:3)	Fa_4^- (5^-)
$Sol_3 : Ut_4$ (3:4)	T^- (T^-)
$Ut_3 : La_3$ (3:5)	Fa_4^- (8^-)
$Ut_3 : Mi_4$ (4:9)	T^- (13^-)

When one makes the auxiliary fork feeble enough in these cases and hears it only as a series of beats and not as a continuous tone, and, furthermore, when this seems in such cases to be the only beating, one can hardly avoid attributing the beats to the auxiliary tone with a summation tone which is present but not audible. The plausibility of this explanation is, of course, strengthened by the results of Krueger's experiment already referred to. His subjects actually heard the summation tones directly.

But against such an explanation of the beats of the auxiliary fork, it may be objected that these beats are not due to the presence of a summation tone at all but that they are explained easily from the fact illustrated above; viz., that generating tones are so closely associated with their difference tones, or resultants, that when the latter beat the former beat also. *E. g.*, in the first case of those given above it may be urged that a differ-

[12] We have not the Re_4 in the laboratory so I could not use the lower octave of this interval.

ence tone 4^- (from $9^- — 5$) interferes with the primary tone 4 and that the beats are located principally in the tone 9^-, the higher of the two primaries.

In addition to this objection another may be raised. It may be urged that, in general, beats cannot be located with any degree of certainty. This seems, to some extent, to be true from the results of Krueger's experiments. A very simple experiment may prove that the assertion made in this second objection has good grounds. Take, *e. g.*, two forks of like pitch, as two Ut_8 forks. Depress one slightly by the method explained above. Now, when these two forks are sounded together we have *one beating tone*. If *any second* tone is sounded simultaneously with this beating tone, the second tone is plainly heard to beat also. This second tone, as is true of the auxiliary above, may be sounded so feebly that only the *beats* are heard. The beats of this tone seem to alternate with those of the first in a fashion that may be represented in this way

Beating tone ——— ——— ———
etc.
'Second tone' ——— ——— ———,

This is very noticeable when the beats are slow. Whether this influence of a beating tone on other tones is or is not merely a psychological one[13] (and it probably is) is a question that has but little bearing on the present use of the phenomenon. The mere fact that there is such an effect produced by beating tones is one that must be reckoned with in all studies in which the localization of beats is important.

These objections make it doubtful whether the beating associated with the auxiliary forks above proves the existence of summation tones. Koenig's theory, as well as others that have been advanced, has no place for the first difference tones of large intervals approaching or exceeding the octave.[14] Now,

[13] It is well known, *e g.*, that a fluctuating intensity of light will subjectively affect other lights which are objectively constant. The above experiment, therefore, need not be regarded as affecting Helmholtz's physiological thory of tonal analysis.

[14] It is true that with auxiliary forks nearly attuned to the first difference tone, Koenig heard a beating for intervals as large as 8 : 15. He says nothing of where these beats *appeared* to be located. Cf. *Quelques Expérience d'Acoustique*, note, p. 130.

since the first objection of the two just considered, might be expected from Koenig, the following possibilities are open to us. We may take as generators two tones so near together in pitch that their summation tone, and hence the auxiliary used to detect it, lies nearly an octave above the higher of them. For this purpose I have used the following intervals:

Primary Tones	Auxiliary Tones
$Ut_3 : Re_3$ $256 + 288 = 544$	Re_4b (17) of 546.1 vib.
$Re_3\sharp^- : Fa_3\sharp$ (nat.) $304.4^- + 341.7 = 646.1^-$	Mi_4 of 640 vib.
$Mi_3 : Fa_3\sharp$ $320 + 362 = 682$	Fa_4^- of 682.6^- vib.
$Fa_3\sharp : Sol_3\sharp$ (nat.) of $362 + 406.3 = 768.3$	Sol_4^- of 768^- vib.
$Fa_3\sharp : La_3$b of $362 + 409.6 = 771.6$	Sol_4 of 768 vib.
$Fa_3\sharp : Sol_3\sharp$ of $362 + 400 = 762$	Sol_4^- of 768^- vib.
La_3b $: Si_3$ of $409.6 + 480 = 889.6$	7^- of 896^- vib.
La_3b $: Si_3$ (nat.) of $409.6^- + 483.2 = 892.8$	7^- of 896^- vib.
La_3 (nat.) $: La_3\sharp$ (nat.) $430.5 + 456.1 = 886.6$	7^- of 896^- vib.
$Si_3 : Re_4$b of $480 + 546.1 = 1026.1$	Ut_5 of 1024 vib.
$La_3\sharp^-$ (nat.) $: Ut_4$ of $456.1^- + 512 = 968.1^-$	Si_4 of 960 vib.

In all these cases *the auxiliary fork was plainly heard to beat.* Many of the intervals, being dissonant, beat violently, but by varying the degree of depression of the auxiliary fork (or of one of the primaries as the case may be) its beating was easily discriminated from the other, more rapid, beating of the dissonant primary tones. Facts of this kind are not easily reconciled with a view like that of Koenig, *e. g.*, which denies the existence not only of summation tones but also of the first difference tone when the intervals are large. The summation tone, then, as indeed Krueger's results go to show, seems to be present with practically all intervals whether the vibration ratio is simple or not.[15]

Of course, *our* results are valid (if at all) only for very small intervals. Krueger, however, studied large intervals, as well as small ones.

Now, in the light of Krueger's results, it may be urged against the above evidence for summation tones, that intermediate difference tones and hence difference tones of large

[15] When this ratio is very complex an extremely high order of upper partial tones must be taken to give a difference tone equal in frequency to the summation tone of the primaries. Even the ratio 8 : 9 requires for this purpose, the presence of the 17th partials.

intervals do exist, whereas the above argument posits their non-existence. This objection is a valid one. But it is to be remarked that only those theories which already admit the existence of summation tones will urge this objection. With such theories we have no quarrel. All theories, however, which deny the existence of summation tones also deny that of difference tones of large intervals.[16]

The immediately foregoing statement is hardly necessary after the excellent work of Krueger. It is excusable, however, from the fact that Krueger studied summation tones only incidentally. But now we come to the important part of our investigation. *Are these supposed summation tones to be explained as difference tones of higher 'orders'?* This question Krueger has also settled, it seems to me, in case of all summation tones which are very weak. His generators were practically free from overtones, yet his observers frequently called attention to weak summation tones. But with certain intervals, *e. g.*, the fifth and the fourth, the summation tones are not hard to hear when the interval is not pitched high. Koenig admitted that the summation tones of these intervals had been heard directly in case of siren tones. Since these tones clearly possessed overtones he supposed that the summation tones generated by them were really 'beat-tones' of upper partials. Rücker and Edser showed that they are not such beat-tones. In later years, even such a close adherent to Helmholtz's view as K. L. Schaefer has supposed that summation tones which are 'audible under ordinary conditions' may be due to the presence of upper partials, *i. e.*, that in cases where they are perceptible with the unaided ear they are really difference tones.[17] Max Meyer, as we have seen, was of like mind. The results that follow refute such notions.

It will be recalled that two quite different possibilities have been suggested for explaining the summation tone as a mere difference tone:

[16] None of these statements can, of course, apply to Meyer's theory. He admits that on the Helmholtzian principle of transformation due to asymmetry in the drum these tones *may* originate in the middle ear. He rightly holds them to be very weak. On his view in this connection see above, pp. 91 ff.

[17] *Supra*, p. 101.

1. The first of these explanations, suggested by Apunn, Preyer, and others, makes the summation tone a difference tone of the 'second order' according to the formula

$$2h - (h - t) = h + t.$$

2. The other explanation, given by Koenig, makes the summation tone a difference tone of the 'first order' resulting from appropriate upper partials according to the equation

$$n(h - t) = h + t.$$

Schaefer's explanation, where this one requires too high orders of partial tones, may be represented thus $nh - mt = h + t$.[18]

The first of these views is closely allied to the one which derives the second difference tone from the first with one of the primaries, in this way:

$$t - (h - t) = 2t - h.$$

The intensity difficulty, as has often been pointed out, at once confronts this view. The following experiment is given simply to illustrate the difficulty. The two forks Ut_4 and Mi_4 were fastened securely to the side of the table in such a position that their resonance boxes pointed in a vertical direction. They were then simultaneously actuated by dropping on their horizontally projecting prongs two small rubber stoppers each weighing 13.2 gr. The stoppers fell on the prongs near the end and immediately bounded off. To prevent any noise a piece of twine was attached to each stopper and held so that the stopper could not fall on the floor. By this means it was possible to regulate fairly well the intensities of the primaries for purposes of comparison. Both of the forks used gave very clear tones without any perceptible overtones for the intensities used. Mi_4 continued sounding longer, however, when the two were actuated with about the same force.

Now it was found, on various tests of this kind, that the *second* difference tone, Sol_3 was easily perceptible when the stoppers dropped upon the prongs of the forks from a position 4.7 cm. above them. The primary tones in such cases were

[18] n and m stand for whole numbers, h and t, of course, for upper and lower primary tones respectively.

themselves very feeble. The second difference tone, it was found by the use of a stop watch, continued to be audible for from 5 to 16 seconds after the forks were actuated. There was yet absolutely no trace of the first difference tone Ut_2. The distance through which the stoppers had to fall was now increased gradually until this tone became audible, too. This occurred when the falling distance became 50 to 56 cm. These figures were confirmed by tests in which the drop-distances of the weights were gradually decreased from a position at which both difference tones were audible to positions where they became inaudible; first the *first*, then the *second* difference tone disappeared. While the second difference tone was smooth and clear and continued relatively long [nearly as long as the lower primary], the lower first difference tone was rough and soon disappeared. With suitable forks, such, *e. g.*, as the new Edelmann set, this experiment could profitably be carried out to considerable length. Various intervals might be tested in different parts of the scale. The above facts, however, seem conclusive against the view that the second difference tone always arises from the first difference tone and the higher primary tone. This view, therefore, cannot be used to refute by analogy as suggested the existence of real summation tones in the Helmholtzian sense.

It is this same interval that Hermann and others used as illustrative of the intensity relations which they considered contradictory to Helmholtz's theory of different 'orders' of combination tones. The objection has less force when urged against his mathematical theory. According to this theory, as has been suggested, all the combination tones arise *directly* from the primaries. The intensity relations for this same interval (the major third) differ with different pitches of the primaries. *E. g.*, when Ut_8 and Mi_8 are used as the primary tones, the first difference tone appears, for me, before the second as the intensity of the primaries increases. When the primaries are Ut_5 and Mi_5 the two difference tones usually appear and disappear together, lasting but a very short time. In this latter case the primaries themselves soon 'run out.' For

different reasons I could not apply so accurate quantitative measurement to these cases as to the one first described.[19]

If the difference tone $2t - h$ cannot be explained in the manner suggested by the equation

$$t - (h - t) = 2t - h,$$

it is even more improbable that Preyer's explanation of the summation tone, *i. e.*, $2h - (h - t) = h + t$, will hold. Here we not only have an upper partial serving as a primary tone, but we have to do with a very large interval as well, *i. e.*, $(h - t) : 2h$. But the explanation is not hard to test.

The summation tone of the interval $Ut_3 : Sol_3$ was found unmistakably to be audible to the unaided ear.[20] The tone of the fork Mi_4^- beats with it very plainly (we disregard here the possibility of other interpretations of these beats). Preyer would explain this summation tone as follows: $Sol_4 - Ut_2 = Mi_4$ [*i. e.*, $2 \times 384 - (384 - 256) = 640$]. Now I sounded very loudly the forks Sol_4 and Ut_2, but heard no difference tone at all. Mi_4^- was then used as an auxiliary tone. The primaries were again sounded loudly while the fork Mi_4^- sounding very feebly was held to the ear. Several tests were made with different degrees of depression of the auxiliary tone. Only *very* faint beats were noticeable,[21] showing that an exceedingly weak difference tone was produced. To be sure that beats were actually heard Mi_4^- was afterward sounded with another Mi_4 fork. The beats had the same frequency. The experiment was repeated with the corresponding tones of the next octave above, *i. e.*, Sol_5 and Ut_3, Mi_5^- being used as the auxiliary. Similar results were obtained. It is to be noted that the summation tone of $Ut_4 : Sol_4$ was not directly audible to those of us who heard it with the interval an octave lower.[22]

The only other test of the kind possible with the forks available was one with the fourth, $Sol_3 : Ut_4$. The sum of

[19] We are very much in need of a careful study of intensities with generators that can be relied upon.

[20] See below, p. 125.

[21] And these *may* have been due to a low difference tone Ut_3^+ (from $Sol_4 - Mi_4^-$) interfering with Ut_3.

[22] I seemed to hear it occasionally after special practice but was always uncertain; the low difference tone was very loud and rough.

the vibrations of these forks is 896, *i. e.*, the vibration number of the fork 7. The summation tone in this case was inaudible, but beat very plainly with the weak tone of 896^- vibrations. The same result was obtained in this case as in those above, viz., the difference tones of the forks $Ut_2 : Ut_5$ (1:8) beat *very* feebly with the auxiliary tone.

In all three of the cases just considered, the forks taken to represent the interval $(h - t) : 2h$ were sounded with considerable intensity. When they give more feeble tones, but certainly louder than the difference tone and the first upper partial for which they are substituted, *purely negative results were obtained:* the auxiliary tone revealed no beats whatever. These few experiments seem conclusive against such an account of summation tones as that given by Preyer and others. The explanation, it is true, is intended to account only for summation tones which are actually audible. We may attach considerable importance to the *one* such case here examined. The result of that one case seems absolutely decisive against the explanation in question.

We shall now consider the second type of view—*i. e.*, that of Koenig—which regards the summation tone as a first difference tone (beat-tone) of upper partials of the same order. Is such an explanation probable, or even possible, in *any* case?

In the first place tests with auxiliary forks for the existence of upper partials contradict the view. The second partial, *i. e.*, the first upper partial, of Ut_3 was audible to the unaided ear. The auxiliary fork beat with partials as high as the fifth inclusive. When the fork Ut_3 was damped by holding one prong near the stem with thumb and a finger, the fifth partial seemed to drop out, leaving only four present. Of Sol_3 the second partial was audible. The auxiliary tone beat with partials as high as the fourth. No fork was available to test for the fifth, but the fourth was so weak that the fifth probably did not exist. Damping does not seem to affect the upper partials as much as it was at first supposed. I am not sure that the fourth partial in this case was eliminated by damping. Of Mi_3 the partials as high as the fourth beat plainly with the auxiliary tone. I did not extend this test to the other forks

used below, but it may be safe from the above results to say that if partials beyond the fifth are present at all, they are extremely weak.[23]

If upper partial tones are effective in the production of combination tones, as some persons have supposed, this fact should easily be discoverable in the case of the second difference tone. As has already been stated the second difference tone (Sol_3) of the interval $Ut_4 : Mi_4$ is very prominent even when the primaries are sounded very feebly. This difference tone is clear and continues nearly as long as the lower of the primary tones.[24] When the primaries are made stronger the *first* difference tone appears. It is rough and lasts but a short time. *Now, when* Ut_5 *is substituted for* Ut_4 *in this interval, the difference tone* Sol_3 *should, as a rule, be even more prominent, if it is dependent upon the upper partial of the lower tone of the interval* $Ut_4 : Mi_4$, as has been supposed from the pitch $2t - h$. But when the substitution is made, this difference tone at once loses its clearness and takes on the characteristics of the *first* difference tone just described. Moreover, the primaries must now be sounded considerably louder for the appearance of this tone. This seems to show conclusively that the *upper partial of* Ut_4 *in the interval* $Ut_4 : Mi_4$ *has practically nothing at all to do with the generation of the second difference tone* Sol_3. Meyer and Krueger by another method have indeed already proved more or less conclusively that combination tones are practically not at all dependent upon upper partials of the primaries, so further tests along the line indicated here would be needless even if the necessary forks were at hand.

What holds for difference tones with respect to upper partials ought also to hold for summation tones—especially if the latter *are* difference tones! But since Koenig and others, and even as recent an experimenter as K. L. Schaefer, have attributed audible summation tones to upper partials, it seems necessary to investigate the matter further.

[23] It is, of course, true that occasionally a very high loud partial is heard, but such cases are usually irregular and the partial may readily be damped out without affecting the combination tones. The experiment following, anyway, rules out the possibility of the effectiveness, for the production of summation tones, of upper partials.

[24] Mi_4, it will be remembered, continues longer than Ut_4.

If the fifth partials of the fifth (2:3) produce an audible difference tone ($5 \times 3 - 5 \times 2 = 5$) it is reasonable to suppose that in most cases partials below these produce even louder difference tones.[25] The second partials should give rise to the tone 2 (*i. e.*, $2 \times 3 - 2 \times 2$) the third to the tone 3; the fourth to 4. These tones all correspond to the primaries or to some of their upper partials. The case is different with the major third (4:5). If we look at the upper partials

4, 8, 12, 16, 20, 24, 28, 32, 36, . . .
5, 10, 15, 20, 25, 30, 35, 40, 45, . . .

it is evident that besides the tone 9 (*i. e.*, $45 - 36$) we ought to hear (other than upper partials and primaries) tones corresponding to 1, 2, 3, 6 and 7, according to Koenig's explanation of summation tones. The tones 1 and 3 are also difference tones of the primaries, however. We can test, then, only for 2, 6 and 7.

When the forks Ut_8 and Mi_8 are sounded loudly together their first upper partials are audible but very rough, due evidently to the summation tone lying between them.[26] This is much less noticeable when the interval is pitched an octave higher, but a very weak auxiliary tone of pitch nearly equal to that of the summation tone beats plainly. Now is there any evidence of the existence of those other tones which we ought to hear if Koenig is right?

Using the primaries $Ut_8:Mi_8$ (4:5) we may test with the auxiliary fork Sol_8^- for the tone corresponding to 6. When the primaries are sounding very loudly and Sol_8^-, sounding very feebly, is held to the ear the tone is actually heard to beat plainly. In this case, however, the beating is no proof of the existence of the tone 6. It is doubtless due here to the interference of two first difference tones as is evident from these numbers:

$$6^- - 5 = 1^- \text{ and } 5 - 4 = 1.$$

It will be recalled that in cases of this kind the beating is very

[25] See the note just preceding this, however.

[26] Cf. Krueger's remarks on the intertone (Zwischenton) in *Phil. Studien*, XVII., 1901.

marked. When the auxiliary tone in this case is sounded more strongly these difference tones are actually heard beating.

In order to test for the tones 2 and 7 we take the primaries an octave higher (Ut_4:Mi_4) and use the auxiliary forks Ut_8^- and 7^- (896^-). Ut_8^- beats slightly but unmistakably with the primaries, but the frequency of these beats is twice that of those which Ut_8^- makes with the other fork Ut_8. The rapid feeble beats heard are due then to the interference of the upper partial of Ut_8^- with the primary tone Ut_4. This is clearly evident when the intensity of the auxiliary fork is increased. When, however, the auxiliary fork is sounding so feebly that upper partials do not occur, *the beats entirely disappear*. The tone 2, therefore, very probably does not exist. On the other hand the auxiliary fork 7^- beats very plainly with the primaries even though it is very weak. This beating, however, is not due to another tone corresponding to 7, but is the beating due to difference tones as shown by the equations

$$7^- - 4 = 3^-$$

and

$$2 \times 4 - 5 = 3.$$

This is easily proved in this way: Instead of depressing the auxiliary fork 7, make Ut_4 (4) flat enough to beat twice in a second, *e. g.*, with another Ut_4 fork. Now the auxiliary fork beats six times a second with the primaries. This is because when the tone Ut_4 (represented by 4 in the equations just given) is depressed two beats, its second partial (2×4) is depressed four beats. An illustration with actual vibration numbers may make this clearer. The beating of the auxiliary fork, 7 (896) depressed to say 894 vibrations per second with the primaries Ut_4:Mi_4 (512:640) is due to the interference of the two difference tones.

$$382 \text{ (from } 894 - 512, \textit{ i. e.}, 7 - Ut_4)$$

and

$$384 \text{ (from } 2 \times 512 - 640, \textit{ i. e.}, 2 \times Ut_4 - Mi_4).$$

If instead of depressing 7 we depress Ut_4 two beats per second, *i. e.*, to 510 vibrations, the beating of the auxiliary tone 7, held

to the ear, has a frequency of *six* per second as is evident from the equations:

$$896 - 510 = 386$$
$$2 \times 510 - 640 = 380.$$

When, however, the tone Mi_4 (640) instead of Ut_4 is depressed two beats per second the auxiliary fork beats only twice per second, as the explanation just given evidently requires. The beating of the auxiliary tones, therefore, which was heard in testing for the tones 2 and 7, corresponding to beat-tones of upper partials of the major third (4:5) does not prove the existence of these alleged 'beat-tones,' but *must* be explained otherwise. That such 'beat-tones,' arising from partials lower than those which give rise (according to Koenig) to the summation tone, do not exist is proved, then, (1) by the fact that in several cases tested the auxiliary tone, intended to interfere with them, does not beat at all; (2) by the fact that whenever the auxiliary tone does beat, the beating is attributable to low difference tones, *and does not have the frequency required by Koenig's supposition.* In the illustration above, where the beating of the auxiliary fork had a frequency of *six* per second when Ut_4 was depressed *two* vibrations, it is evident that if the tone 7 existed at all and was produced by a 'beat-tone' of the seventh partials of the primaries, *the frequency of the beats should have been seven times two per second, since the seventh partials would be depressed seven times as many beats as the first.* On this point we shall dwell more in detail very soon.

We have assumed, then, on very good ground, that if the summation tone be a 'beat-tone' of upper partials, as Koenig suggested, several other 'beat-tones' of lower partials should be even more plainly audible; and it has now been proved conclusively, I think, that such other 'beat-tones' *do not exist.* Aside from the proof here given it should be noted that *these lower 'beat-tones' of upper partials are never audible*[27] *to the unaided ear,* whereas, the summation tone frequently is audible.

[27] At any rate I have never been able to hear them and no one else seems to have done so. Hällström thought he heard a tone $2h - 2t$, as has been seen, but was probably deceived. Of course, this tone may exist when the second

We have yet one more method of attack which meets even more directly the theories here opposed, and which must hold quite independently of the validity of the use of auxiliary tones in establishing the *existence* of summation tones. The principle of this method has already been suggested.

After having convinced myself that the summation tones of the primaries $Ut_3:Sol_3$ (2:3), $Re_3{}^b:La_3{}^b$ (2:3), $Fa_3:La_3$ (4:5) and $Ut_3:La_3$ (3:5) are audible to the unaided ear, I experimented with other students of the laboratory to assure myself that the summation tones were not imaginary. The students selected were all trained in experimental psychology. Those who served in this connection were Miss Elizabeth R. Shaw, Miss Florence E. Richardson, and Miss Grace M. Fernald. Each subject was asked to select from a number of high pitched forks the tones that were audible in addition to the primaries, when certain intervals were sounded. Only one of the subjects was present at a time, and in no case did she know which fork of the group represented the summation tone. The primaries were sounded a little above medium intensity with a rubber hammer. The intervals used were those just named. At least one of the second partials of the primaries was always heard. The fork representing the summation tone was in every case selected. Some of the subjects found it more easily than others. No tone, other than the summation tone or the first upper partial tones, was ever selected as a final judgment, even though in some cases the experimenter called special attention to other tones to try the force of suggestion. In some cases the subject readily sang the note representing the summation tone. It seems evident from these tests that the summation tones of these intervals are actually audible.

Now these summation tones ought to beat with auxiliary tones of nearly their pitch, and on this basis it should be possible to ascertain some facts concerning the mode of origin of these tones. If the summation tone is a difference tone, or a 'beat-tone,' of upper partials this should be revealed by the frequency

partials are strong, but its existence will not affect the above argument. In any case it ought to be very much stronger than the summation tone if Koenig's view be correct.

of its beats with an auxiliary tone. From these numbers, representing the fifth with its upper partials,

$$\begin{array}{ccccc} 2, & 4, & 6, & 8, & 10, \ldots \\ 3, & 6, & 9, & 12, & 15, \ldots \end{array}$$

it is evident that if the tone represented by 2 is depressed one vibration, the second partial, 4, will be depressed *two* vibrations; the third, 6, three vibrations; the fourth, 8, four vibrations; the fifth, 10, five vibrations; and so on. This is easily proved thus: if Ut_8^- beats once per second with Ut_8 it is found to beat twice with Ut_4, three times with Sol_4, and so on. Now if Koenig's explanation of summation tones as 'beat-tones' of appropriate upper partials, be correct, it is evident that *a depression of one vibration of the primaries of the fifth will depress the summation tone five vibrations. From this fact we can make a decisive test of the validity of this explanation.*

In the attempt to test this explanation I soon encountered a difficulty, one, however, which was not insuperable. When one of the primaries is depressed the interval becomes imperfect and the dissonant beats interfere with a careful study of the summation tone, but the beats due to the imperfection of the interval are distinguishable from those of the summation tone with the auxiliary of nearly the same pitch. To get rid of the beats of the imperfect interval the following procedure was adopted. First, after one of the primary tones had been depressed one vibration per second, the auxiliary fork was gradually depressed until the only beats remaining were those of the imperfect interval. In the case of the fifth given above these beats had a frequency of three per second when the *lower* primary tone was depressed one vibration.

This is evident from the following numbers:

384 is the vibration number of the higher tone.

255 is the vibration number of the depressed lower primary tone.

129 is the first difference tone.

126 (*i. e.*, $255 \times 2 - 384$) is the second difference tone.

3 is the number of beats per second due to the interference of these difference tones.

After this adjustment of the auxiliary tone the lower primary tone was again raised to its true pitch. The only beats then remaining were those resulting from the interference of the auxiliary with the summation tone, and possibly also beats of the inaudible difference tone (produced by the auxiliary tone with the upper primary) interfering with the lower primary thus:

$$5^{-} - 3 = 2^{-} \text{ which beats with } 2.$$

Now it is evident that raising the depressed primary tone one vibration to its true pitch should make the difference-tone-beating have a frequency of *one* per second (since the difference does not depend upon any upper partial), while the summation tone, if Koenig's explanation of it be true, should be raised five vibrations and hence should beat *five* times per second with the auxiliary tone. One ought, therefore to hear two sets of beats: (*a*) a slow beating with a frequency of one per second, due to the difference tone, if it is present; (*b*) a rapid beating with a frequency of five per second due to the summation tone. *There is absolutely no trace of such rapid beats, whereas the slow beats are very perceptible and can easily be counted.* The intervals tested in this way are $Ut_8{:}Sol_8$ (2:3), $Re_8{}^{b}{:}La_8{}^{b}$ (2:3), $Fa_8{:}La_8$ (4:5), and $Ut_8{:}La_8$ (3:5). In every case the summation tone was audible to the unaided ear and beyond question should beat with the auxiliary tone in tests such as the one just described. The conclusion seems inevitable, that the slow (the only) beating heard is due, at least in part, probably entirely, to the summation tone, and that consequently *the summation tone is depressed the same number of vibrations as the primary tone is depressed,* in accordance with Helmholtz's view.

These experiments, disproving the valadity of Koenig's explanation of summation tones, apply with equal force to explanations like that of K. L. Schaefer, illustrated by the equation

$$mh - nt = h + t.$$

We are, therefore, justified in the conclusion that the second class of views, regarding the summation tone as a first difference tone of appropriate upper partials is also wrong.

Our conclusion which is in harmony with Helmholtz's theory of combination tones, agrees entirely with the results of Krueger's recent experiments in which he found that subjective summation tones are audible even when upper partials of the primaries are eliminated. On the whole, *experimental evidence contradicts any theory which regards summation tones as dependent upon upper partial tones or difference tones of the primaries.*

Summary of Data Bearing upon the Ohm's Law Question.

Objections to the Ohm-Helmholtzian View.	*Replies and Points Favoring the view.*
1. Upper partial tones actually present are heard either relatively weak or not at all (Seebeck)..	1. Until occasion is afforded to analyze the tones of a clang we perceive *synthetically*. By proper training we can perceive the upper partials relatively louder than usual (Helmholtz).
2. The vibration numbers of difference tones equal the frequency of the beats of the primaries (Koenig, *et al.*).	2. No beats thus correspond to the summation tone (Helmholtz).
3. Large intervals may beat even when there are no overtones present (Koenig, Wundt, *et al.*).	3. Such beats, when there are no overtones to produce them, are due to the presence of difference tones which may be too weak to be directly heard (Helmholtz, Bosanquet, Krueger).
4. 'Interruption tones' are heard when a given tone is periodically intermitted or partially so intermitted (Koenig, Dennert, *et al.*).	4. These tones can be resonated with physical resonators, proving that they correspond to physical pendular vibrations (Schaefer and Abraham).
5. Phase changing vibrations are experienced by the ear as tones (Koenig, Hermann, *et al.*). The ear detects phase differences and	5. These *supposed* phase changing vibrations have been resonated with physical resonators, proving that they are actually sinus-form

employs them in localization of sound (Rayleigh).	(Schaefer and Abraham).
6. Shifts of phase in upper partial tones of a clang affect the quality or timbre of the clang (Koenig). The change in timbre is due not to shift of phase but to change in objective form of the *compound wave* (Hermann).	6. It is probable that the instrument used (the wave-siren) did not reproduce with accuracy these shifts of phase (Hermann); anyway the change in timbre is but slight. The second statement has never been proved.
7. The intensities of combination tones are not experienced in the proportions required according to the Helmholtzian theory (Voigt, Stumpf, Hermann, Meyer, *et al.*).	7. (Not satisfactorily answered by any theory.)
8. A low tone may obliterate a higher one but the reverse is not true (A. M. Mayer).	8.
9. Two tones conducted separately one to each ear can be heard to beat (Cross and Goodwin).	9. We cannot be quite sure that by means of bone conduction, or by some other mode of conduction, both tones did not operate in the same ear.
10. (The statement opposite this space is of course not necessarily contradictory to the opposition against Ohm's law.)	10. Helmholtz's theory of objective combination tones is supported by the fact that such tones have been resonated with various physical resonators (Helmholtz, Rücker and Edser, Forsyth and Sowter, Schaefer).
11. The intertone (Zwischenton) is contradictory to a strict interpretation of Ohm's law.	11. It is explained without serious difficulty on the basis of 'physiological adaptation of nerves' (Stumpf). This tone is not so clear and distinct as might be supposed from some accounts; it is more nearly a 'beating complex' than a tone (Krueger, Peterson).
12. Only such summation tones exist as are attributable (as difference tones) to upper partials	12. Very weak summation tones can in most cases be heard on sufficient practice, or training of

either wholly (Koenig) or in part (Appunn, Preyer, *et al.*).

the ear, and attention (Krueger); 'objective' summation tones are not explicable as difference tones (Rücker and Edser); 'subjective' summation tones are not explicable as difference tones (Peterson).

13. Both 'objective' (Rücker and Edser) and 'subjective' (Krueger) intermediate (zwischenliegenden) difference tones exist.

TABULAR STATEMENT OF THE EFFICIENCY OF THEORIES DISCUSSED IN THIS THESIS.

Fact to be Explained.	Ohm-Helmholtz.	Young-Koenig.	Hermann.	Helmholtz-Wundt.	Helmholtz-Ebbinghaus.	Meyer.
Analysis of complex clangs....................	×	?	?	×	×	×
The common phenomenon of beats.............	×	×	×	×	×	×
Beats of large intervals of pure tones..........	×	×	?	×	×(?)	×
Common difference tones......................	×	×	?	×	×(?)	×
Objective combination tones...................	×	?	?	×	×(?)	×(?)
Intermediate first difference tone (subjective)...	×			?	?	?
Upper and lower 'beat-tones' (?)..............	×(?)	×	×	?	?	×(?)
Summation tones..............................	×			?		?
Intensity relations of combination tones.........	?	?	?	?	?	?
A lower may obliterate a higher tone...........						×
The ear experiences phase differences (?).......	?	×	×	?	?	×
Two tones heard simultaneously, one by each ear (?), beat..............................	?	?	?	×	?	?
The phenomenon of the intertone (Zwischenton)	×(?)		?	?		?
"Tone islands," *Diplacusis binauralis dysharmonica* (pathological phenomena not mentioned in this thesis)..............................	×		?			

A cross (×) means that the theory can explain the fact opposite which it occurs.

A question mark (?) signifies either that the efficiency of the theory is doubtful or that the theory needs modification or supplement.

Note the inconsistencies in Wundt's and Ebbinghaus's theories pointed out in the text.

APPENDIX A.

An experiment reported recently by Exner and Pollak[1] is somewhat closely allied to that of Mayer, and may be mentioned here. I quote from the succinct statement of Bentley and Sabine:

"They purpose to test the resonator theory of audition by using simple tones with periodic reversal of phase. They reason as follows. When a wave train acts on a properly tuned resonator, the effect up to a certain limit is cumulative; *i. e.*, each successive wave increases the sympathetic vibration of the resonator until the limit is reached. If, however, the wave suddenly changes phase, its energy will be directed against the inertia of the resonator, and the two will oppose one another until equilibrium is reached, after which the wave will again produce on the resonator its former cumulative effect. If, now, this change of phase is made periodically, it should result in a wave with much smaller amplitude than the original wave, periodically varying in intensity, unless the phase changes follow one another so closely that the wave is entirely annihilated. Hence it should follow that, if audition is mediated by a series of resonators, a tone thus interrupted should be discontinuous and we should hear bursts of sound alternating with periods of silence. It should further follow that, by keeping the intensity of the tone constant and increasing the frequency of the phase changes, we can cause the tone to decrease in intensity until it entirely disappears. That is, the cumulative effect on the basilar resonators of the waves following between any two successive phase changes will not be sufficient to raise the nervous impulse above the limen of sensibility. If, now, the number of phase changes is kept constant and the physical intensity of the tone is increased, the tone which has become just inaudible should be lifted over the limen.

"Exner and Pollak used three forms of experiment to obtain the conditions which they required: (1) a tuning fork rotated about the longitudinal axis of its stem and having, there-

[1] Sigm. Exner u. Jos. Pollak, 'Beitrag zur Resonanztheorie der Tonempfindungen,' *Zeitschr. f. Psychol.*, XXXII., 1903, pp. 305–332.

fore, four phase changes for each revolution; (2) a stationary fork which actuated a telephonic diaphragm under a current which was periodically reversed by means of a rotating commutator, thus causing two changes of phase at each revolution; (3) a rotating stopcock which brought alternately to the ear the waves from the side and from the face of a continuously sounding fork. The results reached by these methods confirm the authors' hypothesis regarding auditory resonance. They found that the sudden reversal of phase, when it comes with sufficient frequency, destroys the tone. A critical rate of phase change was discovered [*e. g.*, 10 vibrations between each two successive phase changes for a tone of 240 vibration]. At this rate (which was fairly constant under the given conditions) the sound of the tuning fork disappeared, and reappeared only when the rate of revolution was diminished."[2]

Bentley and Sabine repeated the experiment under somewhat similar conditions.[3]

Their results did not agree entirely with those given above. They promise another article which, I believe, has not yet appeared.

Dr. Geo. E. Shambaugh has recently made a very careful study of the tectorial membrane in the pig's ear.[4] He found that this organ, whatever its function, increases enormously in size from the base of the cochlea toward the apex, and suggests that this structure may serve as the resonant analytic organ in hearing. It is doubtful, however, whether this organ, despite its fibrillar structure, can act as a resonator. No physical resonators of this kind are known, or described by physicists. Dr. Shambaugh finds that at the base of the cochlea the basilar membrane is completely underlaid by a bony bridge.

K. Kishi,[5] on the other hand, finds that the basilar membrane is not so unfavorable, in its structure, to a view like that of Helmholtz.

[2] *Am. Jour. of Psychol.*, XVI., 1904, pp. 489–90.

[3] 'A Study of Tone Analysis,' *ibid.*, pp. 484–98.

[4] George E. Shambaugh, A Restudy of the Minute Anatomy of Structures in the Cochlea with Conclusions bearing on the Problem of Tone Perception, *Amer. Journal of Anatomy*, 1907, VII., 246–257.

[5] K. Kishi, 'Cortische Membran und Tonempfindungentheorie,' *Pfluger's Archiv*, CXVI., 1907, 112 ff.

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